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GRAPHICAL DETERMINATION OF STRAINS IN TRUSSES WITH PARALLEL CHORDS.

By CHAS. H. TUTTON.

Written for Van Nostrand's Magazine.

Believing the following to be a simpler method of finding the strains in Trusses with Parallel Chords graphically than any which has fallen under the writer's observation, it is here presented to the public, in the hope that it may do its mite of good. It will be presupposed that the reader is familiar with the principle of the lever, and the "triangle and polygon of forces." Sufficient extension of the latter for our purpose, will be found in Prof. DuBois' article in Van Nostrand's Magazine for Feb., 1875.

We will consider two cases.

1ST. PRATT TRUSS WITH VERTICAL ENDS.

Let CDEA Fig. 1 represent a Pratt truss with vertical ends. For the purpose of allowing our results to be verified by calculation, we will suppose it to consist of 8 panels of 10 feet each, having a moving load of 8 tons, and a dead load of 2 tons per panel per truss. Height of truss 10 feet. Scale of tons and feet, 20 to an inch. Heavy lines indicate compression, light lines tension, and dotted lines those of construction.

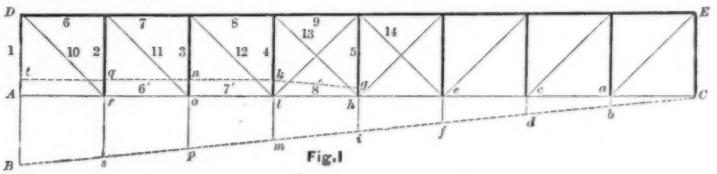


Fig. 1

A. MAXIMUM STRAINS IN CHORDS.

We will assume the fact as already proven, that the maximum chord strains occur when the truss is fully loaded. To find these strains, erect an indefinite perpendicular to the horizontal line, OZ, and on it lay off the reaction of the left abutment, OL, which, as the load is symmetrical with respect to the center of the truss, will be one-half of the weight of the truss and its load, or $8 \times 10 \div 2 = 40$ tons. Since one half of one panel weight is supported immediately by the abutment, and therefore causes no strain on the truss, lay down from L to M this half panel weight, to the scale of weights equal to 5 tons, and then consecutively, the equal panel weights of 10 tons = MN,

NP, PR to O, for the "force polygon" (See Du Bois' article before referred to). RO then becomes but 5 tons or one half panel weight, and this is as it should be, since the load at h (Fig. 1) is equally divided between the two abutments, being at the center of the truss. The reaction of the abutment, less the half panel weight immediately supported by it, is evidently the compressive strain on post 1. This strain is in equilibrium with the strains through brace 10, and top chord 6, the three pieces which meet at the point B, hence from M draw a parallel to brace 10, (Fig. 1), and limit it by the horizontal through O, and we find 6 as the compression in the top chord, and 10 as the tensile strain on the corresponding brace, caused by an uniform load on the truss. Passing to a (Fig 1), we find this latter strain in equilibrium with the strains through bottom chord 6′ and post 2, but we know that one panel weight of the load does not affect post 2, therefore we draw, in Fig. 2 the vertical 2 parallel to post 2, and limit it by a line through N parallel to the bottom chord of the truss, or horizontal. This gives us 6′ as tension in the lower chord and 2 as compression on post 2. Continue these operations until the diagram is completed. It can be checked by a similar construction from the lower side.

We have then for maximum compression on top chord, on 6, Fig. 1=6 or OX Fig. 2; on 7 Fig. 1=7 or OY, Fig 2; on 8=8 or OZ; on 9=9 or OM′. For maximum tension on lower chord, on $Ar=Ca=0$ since there is no horizontal through M; on 6′=6′; on 7′=7′; on 8′=8′, and on pin connection at the center of the truss=9′=OM.′

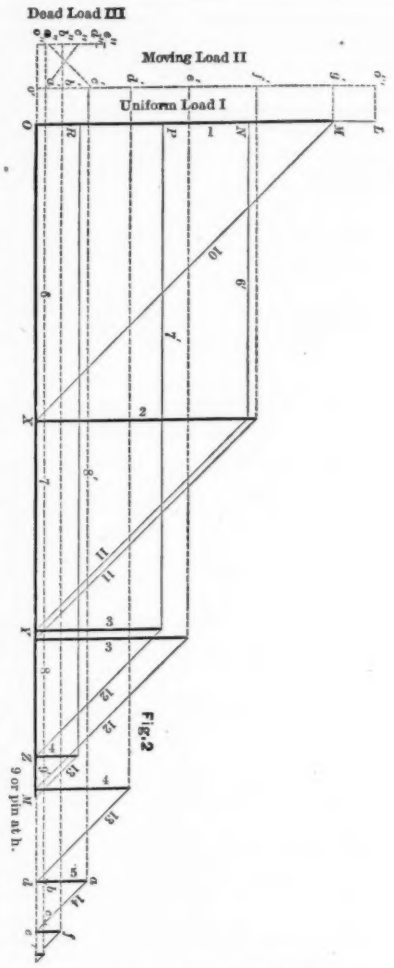


Fig. 2

B. ROLLING LOAD, MAXIMUM POST AND BRACE STRAINS.

Suppose the bridge entirely covered, and then that the load moves off to the right. It is self evident that with the bridge covered, the strains on post 1, brace 10, and top chord 6, will be the same as before, since the reaction of the abutment is the same, hence, as before, on the second vertical marked "running load" in Fig. 2, lay of $0'0'''$ equal to reaction of abutment=40 tons; deduct $0'''g'$ equal to an half panel weight of total load or 5 tons, and draw 10, limiting it as before. (All of these distances should be laid off on the first vertical, hence they are all projected on it. They are laid off on different verticals here to avoid confusion). Now in Fig. 1, lay off AB equal to one panel weight of moving load or 8 tons, and draw CB, then will the ordinates at the panel points show the portion of the panel load sent to the left abutment from that point. Also, lay off At=bridge panel weight or 2 tons, and draw tk parallel to AC. At the middle point of this (even paneled) truss, one-half of the dead load goes to each abutment, as is evident from the

chord strain diagram, therefore, lay off at the center, hg= one half a panel load of dead weight or 1 ton. Connect k and g, then will the ordinates between this line and the bottom chord, represent the dead load sent to the left abutment from each panel point. As the live load runs off to the right, the reaction of the left abutment will be diminished by qs, np, km, gi, ef, cd and ab successively, hence from g', Fig. 2, lay down successively $g'f'=qs$; $f'e'=np$; $e'd'=km$; $d'c'=gi$; $c'b'=ef$; $b'a'=cd$; $a'o'=ab$, which should close at o'. Through these points draw horizontals, and complete the diagram similarly to that for an uniform load. This gives the maximum strains on braces and verticals.

C. COUNTER BRACES.

Certain braces found by the above method to have strains indicated on them are unnecessary, as the dead weight of the bridge, acting through the main braces and in opposition to these, will more than counterbalance them, since their strain would but tend to relieve the main brace of some of its strain. To determine then, what counters are unnecessary, and, when necessary, what portion of their strain is relieved by the action of the main braces, erect a third perpendicular $o''e''$, equal to the reaction of the abutment due to dead weight only, or $2\times8\div2=8$ tons; then lay off the $\frac{1}{2}$ panel weight $e''d''=1$ ton, immediately supported by the abutment, and thence the consecutive panel weights of two tons, similar to that for uniform loading. By referring to Fig. 1, we see that the strain through brace 10 has no counter strain, no portion of the load held immediately by abutment A going to the right. That through brace 11 is countered by strain through brace corresponding to machine $o'a'$ in moving load diagram. That through brace 12 by $o'b'$; through 13 by $o'c'$; or, referring simply to abutment reactions, $o''c''$ counters $o'a'$, and hence is connected with it on the diagram. But $o''c''$ is greater than $o'a'$, hence the counter strain only relieves the dead weight strain, and no counter trace is necessary. Similarly, $o''b''=o'b'$, or the two strains neutralize each other in the diagram at f. Again $o''a''$ is less than $o'c'$, hence the counter strain is but partially relieved by the main brace strain on 13; so, drawing the horizontal $a''a'bc$, we find the strains ce and bd due to dead weight, acting in opposition to strains ae and ad of post and counter brace caused by moving load, thus leaving the maximum final strains on post 5 and counter 14 indicated by ab and ac. There being no other strains traveling to the right from dead weight, all other strains are correctly indicated by the diagram for moving load.

The diagram may be checked by a corresponding construction from the lower side.

2D. LINVILLE TRUSS WITH VERTICAL ENDS.

Let us take the worst possible combination, a truss having an odd number of panels, with an odd number on either side of the center panel.

This truss can be divided into two simple systems, the system 1 being shown in Fig. 5. The panel load signifies one panel load of the double truss.

We will take this truss 10 feet high, and 55 feet long. divided into 11 panels of 5 feet each (Fig. 4). Loads and scale as before. The simple system shown there, gives 5 panels 10 feet long, with an end panel 5 feet long.

The load is now eccentric and the reactions of the abutments will no longer be alike.

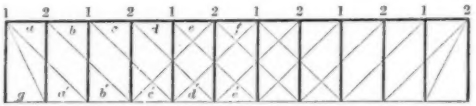


Fig.4

A. TRUSS UNIFORMLY LOADED. MAXIMUM CHORD STRAINS.

We will assume that the half panel load, which is immediately supported by the abutment, is at the end having the short panel, hence, each truss will hold an equal load, symmetrical in opposite directions, or in other words, if we change one system, end for end, we have the other.

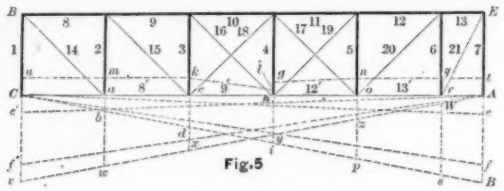
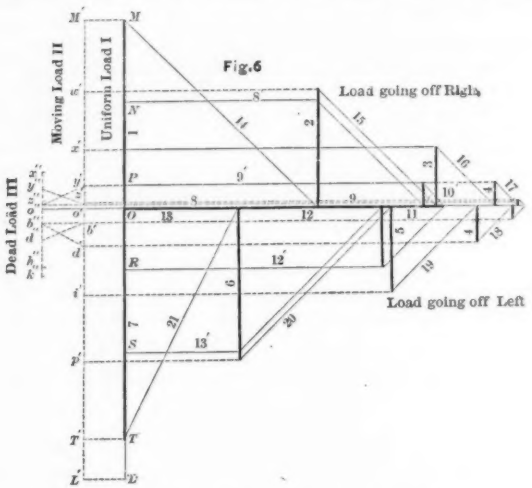


Fig.5

The reactions of the abutments may be found thus: At A, Fig. 5, lay off AB =one panel weight of combined live and dead load, and draw CB. The reaction of the right abutment (exclusive of the half panel load, or 5 tons immediately supported by it) will then be equal to $ab+cd+hi+op+rs$. Similarly for the left abutment, lay down $Cv=CB$, and its reaction will be $rW+oz+hy+cx+aw$. Erect the vertical LM (Fig. 6), and lay off on it OM equal to the reaction of the right abutment, and OL equal to the reaction of the left abutment $=(2+4+6+8+10)\frac{10}{11}+5=32.27$ tons. TL will then be the half panel load, or 5 tons immediately supported, and ML should just equal the total weight of the system or 55 tons. One therefore equals $(1+3+5+7+9)\frac{10}{11}=22.73$ tons, which added to 32.27 gives 55 tons. (Since the truss is not symmetrical, the strains will not



be so, hence we must construct the diagram for each end of the truss, as explained in the Pratt and shown in the figure. As a check, the strains in the chords should be equal at the panel point which send part of its load each way, or 10=11). From M and T lay off the regular panel weights = 10 tons, then construct the diagram as before explained.

This gives the chord strains in one system. The systems being similar, their strains will be similar but from the opposite end. Thus from Figs. 4 and 6. Strain in $g=o$; in $a'=13'$; in $b'=13'+8'$; in $c'=12'+8'$; in $d'=12'+9'$; in $e'=11+9'$, and similarly for the top chord.

B. ROLLING LOAD, MAXIMUM POST AND BRACE STRAINS.

The strains from a moving load will also be different for loads going off in different directions.

As before, lay off Af and Cf' (Fig. 5,) =one live panel load, or 8 tons, and draw the lines Af' and Cf. Also lay off $At=Cu=$one dead panel load, or 2 tons, and draw wk and tu. Then hg and hl may be calculated or found graphically.

Since the dead load is just one-fifth of the combined load, the reactions will be one-fifth as much, or 6.454 and 4.546 tons, whence hl=4.546−4=.546 tons, and hg=6.454−5=1.454 tons, the one ton being supported at the abutment. Graphically, since we know the reactions of the abutments for an uniform load, and know that they can be no greater than that, lay off M′O′ and O″T′ exactly as for an uniform load. Then from M′ lay off M′w′=ordinate included between Af and AC at a (Fig. 5)+am; then $w'x'$=ordinate between same lines at c+ck. Then from O′ lay off ordinate between same lines at r; $z'y'$=ordinate at o; then should $x'y'$ equal ordinate at h+hl, if work is correct. Or, draw the lines Ae', Ce, when Ae=Ce'= one panel load of dead weight or 2 tons, and construct reactions of abutments exactly as for an uniform load, and as is done on vertical 3 for dead load in Fig. 6, then will $o''z''$=lh and $o''b''$=hg, lh+hg should just equal one panel load of dead weight=$z''b''$. Similarly divide up O′L′ from the other reaction, and complete the diagram exactly as before, and we obtain the maximum post and brace strains with load coming on from either end.

C. COUNTER BRACES.

Lay off the dead load reaction on vertical 3 as before explained, then we find $o''y''$ countering $o'z'$ and no brace is needed, $o''z''$ counters $o'y'$ hence counter 17 is needed with strain as given between arrow heads and point when 17 and 4 meet. Similarly for post 4.

On the lower side, the load moving off in the other direction, we find $o''d''$ counters $o'b'$ or no brace is needed, $o''b''$ counters $o'd''$ or counter 18 is needed. also post 4.

The proper value to give to the strain on post 4 is evidently taken from that side of the diagram on which the strain is heaviest.

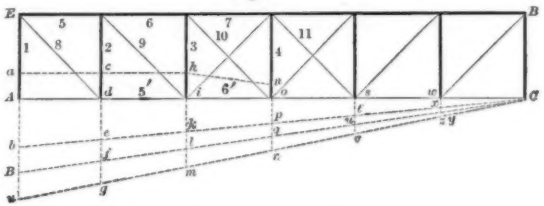
Strains caused by an excess of load in any panel or panels, such as those of a train headed by an engine.

1ST. PRATT TRUSS WITH VERTICAL ENDS.

Let ACBE (Fig. 7), be a Pratt truss with vertical ends, having 6 panels 10× 10. Loading per truss per panel as follows: Dead weight of bridge 3 tons; Uniform live load 6 tons; Excess of engine weight on two panels, 3 tons per panel, or total weight of engine 9 tons per panel. Scales as before.

In this case the reaction of the abut-

Fig. 7

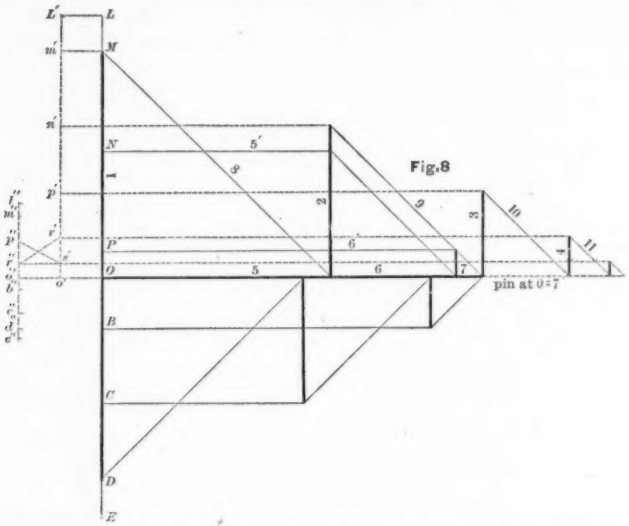


ments will no longer be alike, and our main problem is to find them.

A. TRUSS COVERED WITH LOAD, MAXIMUM CHORD STRAINS.

Suppose the excess of weight to be that due to an engine or engines leading the train, and covering n panels, the whole number of panels in the truss being N.

In this case, therefore, N=6 and n=2. Suppose the train is standing on the truss in such a manner that the last brace, 8 in example, receives the full load of engine excess, and yet so that the excess throws no direct weight upon the abutment A, the panel points d and i will then be loaded with engine excess in addition to the weight acting at all the other panel points. Now when the truss is fully loaded, lay off on the vertical I (Fig. 8), OL=reaction of abutment A. To find this reaction, lay down AB (Fig. 7)=one panel load of combined uniform and dead load or 9 tons, and Bu=excess of engine weight over moving load of uniform density in one panel or 3 tons, and draw BC and Cu. The reaction OL will then be equal to one half the weight of truss and uniform load $+\frac{ne}{2N}(2N-n-1)$ less the one half



panel load supported at the abutment of combined live (uniform only) and dead load. This on the supposition before stated that the engine throws no weight directly on A to be then supported. Formulating the above, let N=number of panels in the truss,

W=weight per panel of dead and uniform load, e=excess of engine weight over uniform load per panel, n=number of panels covered by engines; then, if R =the reaction at A and R′ that at C causing strains in the truss,

$$R=OM=\tfrac{1}{2}W(N-1)+\frac{ne}{2N}(2N-n-1)$$

=for this case 27 tons,

$$R'=OD=W(NI)+ne-R'=24 \text{ tons.}$$

since W=9, N=6, n=2 and e=3.

The total reactions OL and OE are respectively $\frac{1}{2}$W greater.

Graphically, the reaction at A=OL $=\frac{1}{2}NW+gf$ if only one panel is excessively loaded; if two are so loaded=$\frac{1}{2}$NW $+gf+ml$; if then=$\frac{1}{2}NW+gf+ml+gr$, &c. Also OE=EL−OL=(total load of all kinds=NW+ne)−(OL=reaction of the other abutment). Graphically for this particular case, OL=$\frac{1}{2}W+df+il$ $+og+su+wy+fg+lm$, and OE $=\frac{1}{2}$W $+df+il+og+su+wy+yz+uv$. Having thus found OL and OE, we lay off LM=DE=$\frac{1}{2}$W=$\frac{1}{2}$ panel weight of uniform and dead load, then lay off MN =NP=W+e=12 tons, and generally lay off W+e uniformly for n times, that is, as many times as there are panels covered by the excessive load; and then the uniform panel loads = W = PB =BC=CD=9 tons, which should close at D. Then complete the diagram exactly as before explained, and we get the maximum chord strains.

The truss of course will be proportioned symmetrically to allow for the load coming on from either end, hence, the lower half of the diagram is only useful as a check.

B. ROLLING LOAD, MAXIMUM POST AND BRACE STRAINS.

O′L′ and L′M′ are found and laid off exactly as in the last paragraph. Lay off AB in Fig. 7=1 panel load of uniform rolling load or 6 tons and draw Cb. Now as the engine and train backs off towards the right, as the engine passes each panel point it will lessen the reaction of the left abutment by an amount which equals $\frac{ne}{N}$ for each point passed, until the engine gets to the nth post counting from the other abutment, besides the amount of the uniform and dead load. We see this from the fact that if the excess is on d and i, then $fg+lm$ goes to the left, but if moved to

i and o, $lm+qr$ only goes to the left. Their difference $fg+lm-(lm+qr)=fg-qr=\frac{2e}{N}$ or, as we have supposed $n=2=\frac{ne}{N}$. Hence, lay down from M′, $M'n'=cd+de+uv$ ($uv=\frac{ne}{N}$ in this case), $n'p'=hi+ik+uv$; $p'r'=no+oq+uv$; $r's'=st+uv$; but here, at the next panel point, a part of the excessive load has passed off the bridge, and the part remaining throws but yz to the left. Hence lay down $s'o'=wx+yz$ which should close at o'. These distances numerized are equal to 9, 8, 5.5, 3 and 1.5 tons, or total =27 tons. We thus see that after reaching the nth post from the other abutment, the vertical end post being considered as $n=1$, the value of $\frac{ne}{N}$ becomes successively $\frac{n-1}{N}e$, $\frac{n-2}{N}e$ $\frac{n-n}{N}e$ $=o$ as the train leaves the bridge.

Complete the diagram exactly as before for the strains.

C. COUNTER BRACES.

The reactions for the dead load of the bridge being equal on the two abutments, lay off on the third vertical the dead loads exactly as described for the first case, and proceed as there explained.

2D. LINVILLE TRUSS WITH VERTICAL ENDS.

We will take the same truss that we had before, and of which one system is shown in Fig. 9. We will also suppose four panels = n panels of the double truss loaded with an excess of 2 tons per truss per panel. The reactions will now be different from the preceding case, and will also differ when the engine is at different ends of the bridge, hence we must construct two diagrams, one for each case.

A. TRUSS COVERED WITH LOAD. MAXIMUM CHORD STRAINS.

In Fig. 10, suppose the engines to be standing on the left end of the bridge. Let (Fig. 9) CK = one panel weight of truss with uniform load, and KL=H1= the engine excess in one panel. That is CK=EH=10 tons; KL=H1= 2 tons.

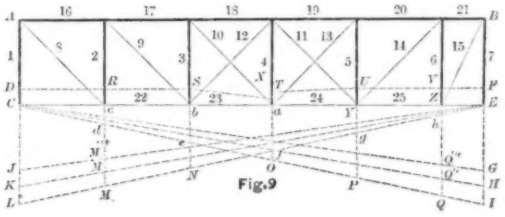


Fig. 9

Now since $n=4$ panels of the double truss are bearing this excess, there will be $\frac{n}{2}$ panels in each single system so covered; in this case=2. (With n even, opposite ends of each system will always support $\frac{n}{2}e$, but if n is odd, the end of the single system with the short panel will hold $\frac{n+1}{2}e$, while the other end would hold $\frac{n-1}{2}e$; that is, n even, $\frac{n}{2}$ panels are loaded in each system with the excess; n odd, the system with the short panel end is loaded at one more panel point than the other). Supposing, however, n even, then in Fig. 10 which supposes the engine at the left abutment, R = MO=M″c+N″b+j″a+g″Y+h″Z +MM″+NN″.

(The M″,N″,f″, &c., are supposed to be on the line KE, immediately above the same letters on the line LE. The line JE will be denoted by thirds, as M‴, N‴, f‴, &c., and similarly for CH and CG. Also CJ=EG=live uniform panel load=8 tons, and JK=GH=dead panel load=2 tons).

That is, numerizing, R=MO=$(9+7+5+3+1)\frac{10}{11}+(9+7)\frac{2}{11}$=25.64 tons.

R′= OL=$\frac{1}{4}$NW−R=cd″+be″+ao″ +YP″+ZQ″+dd″+ee″+$\frac{1}{2}$W.

$=(2+4+6+8+10)\frac{10}{11}+(2+4)\frac{2}{11}+5$ =33.36 tons.

$=\frac{1}{2}$NW+$\frac{1}{2}ne$−R=59+25.64.

N=number of panels in double truss.

Now lay off OM=K, then MN=NP =W+e=12 tons for $\frac{n}{2}=2$ times, then W=10 tons successively to close at V. VL then should equal $\frac{1}{2}$W. Complete the diagram as before.

If, as in Fig. 11, the engine stands at the right end of the bridge, then the panel points Y and Z would bear the excess of load, and OL would equal $\frac{1}{2}$W +ZQ″+YP″+ao''+be''+cd''+QQ″+ PP″. Conversely oY=M″c+N″b+$f''a$ +g''Y+ $h''z$+hh''+ gg'' or, numerizing, OL=5+(2 + 4 + 6+8+10)$\frac{10}{11}$+(8+10) $\frac{2}{11}$=35.543 tons,

OY=(9+7+5+3+1)$\frac{10}{11}$+(3+1)$\frac{2}{11}$= 23.457 tons.

Lay off ML=$\frac{1}{2}$W, and then MN=NP= W+e=12 tons for $\frac{n}{z}$ times, then PR=RS =SY=W=10 tons, which should close at Y. Then complete the diagram exactly as before.

We find by this that either method of loading gives the same center strain on the chords, so long as n is even, but that in the end panels, the strains are heavier according to the position of the excessive load; therefore, from the maximum strains, find the strains in the double truss as before explained.

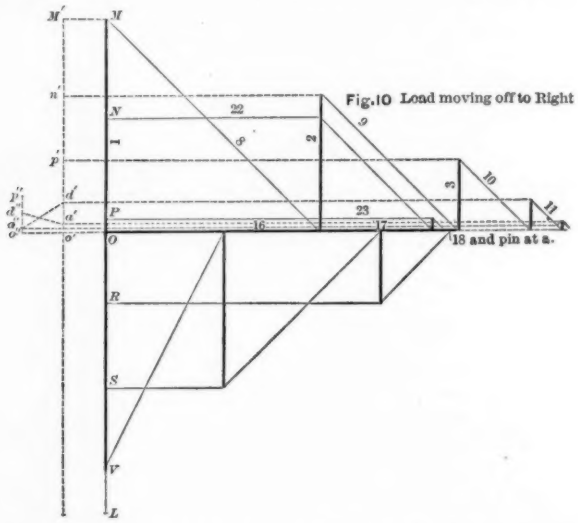


Fig. 10 Load moving off to Right

B. MOVING LOAD. MAXIMUM POST AND BRACE STRAINS.

A. LOAD MOVING OFF TO THE RIGHT.

In Fig. 10 O′M′=OM as before. Also, the strains will be lessened by $\frac{ne}{N}$ as explained previously, N and n still referring to the double truss, hence lay off M′n′ =Rc+cM‴+ee''=9.27 tons; $n'p'$=Sb+ bW‴+ee''=7.82 t; $p'd'$=ax+uf'''+ee'' =4.91 tons; but here, since ZE is put on half panel, $\frac{1}{n}$ of the engine will have left the bridge, or $\frac{ne}{N}-\frac{n1}{N}e$, hence make $d'a'$ =Yg'''+gg''=2.73 tons, and $a'o'$=zh''' +hh''=.91 tons, which should close at o'. Complete the diagram as before.

b. LOAD MOVING OFF TO THE LEFT.

In Fig. 11 O′M′ still equals OM, and the strain caused by e still diminishes by $\frac{ne}{N}$, therefore lay off M′N′=ZQ‴+ee'' +Vz=10 tons; $n'p'$=UY+Yp'''+ee''= 8,545 tons; $p'd'$=Ta+aO‴+ee''=6,544 tons; $d'a'$=be'''+ee''=3.636 tons; and here again the engine goes off at the next panel, so that $a'o'$=cd'''+dd''= 1.817 tons; which should close at O′. Complete this diagram, and assume those strains which give the largest results.

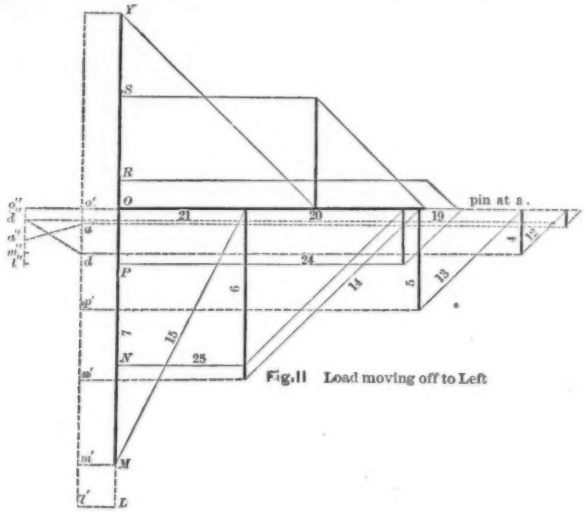


Fig. 11 Load moving off to Left

C. COUNTER BRACES.

These are found precisely the same as when we supposed the bridge covered with an uniform load.

INDUSTRIAL CHEMISTRY.*

By Prof. ABEL, F. R. S.

From "Nature."

THE epoch is well within the recollection of chemists of my generation, when the British iron-master first awoke to the benefits which might accrue to him from an application of the labors of the analytical chemist in connection with iron-smelting.

When the last great stride was made in the manufacture of cast-iron by the introduction of the hot blast, the iron-smelter was naturally led to seek profit, to the fullest extent, with respect both to the great increase in the rate of production of pig-iron attainable thereby and to the economy achievable in regard to the proportions and characters of the materials employed in the production of pig-iron. But after a time the great falling-off in the quality of a large proportion of the products of the blast-furnace, and the difficulties experienced in the production of malleable iron of even very moderate quality, aided by the great impetus to competition in respect of quality, given by the first International Exhibition in 1851, directed the attention of our more enlightened iron-masters to the likelihood of their deriving important aid from chemical science, and more especially from the investigations of the analytical chemist.

Among the earliest to realize the importance of trustworthy and detailed information regarding the composition of the iron ores of the country was Mr. S. H. Blackwell, who, in presenting to the Royal School of Mines a very extensive and interesting series of British ores which he had collected with great labor and expense for exhibition in 1851, placed at the disposal of Dr. Percy the requisite funds for engaging the services of competent analysts (Messrs. J. Spiller and A. H. Dick) who, under his direction

* Abstract of an address before the Chemical Section of the British Association.

and with subsequent pecuniary aid from himself and from Government funds, carried out a very careful and complete examination of this series, the results of which have been of great value, for purposes of reference, to those actively interested in the iron industry. It was, however, the first connection of Messrs. Nicholson and D. S. Price and of Mr. E. Riley with two of the most important iron works of this country, about a quarter of a century ago (*i.e.* at the time when the above investigation was commenced), that marked, I believe, the commencement of systematic endeavors to apply the results of analytical research to the improvement and regulation of the quality of the products of our iron works.

It is, perhaps, but natural that the primary object sought by applications of the knowledge of the analytical chemist should have been to eliminate or reduce the existing elements of uncertainty in obtaining the most abundant yield of pig-iron capable of conversion into railway-bar sufficiently good to meet the minimum standard of quality, and to reduce still further the cost of production of such bar-iron by utilising materials concerning the composition of which (richness in iron, &c.) the iron-smelter was completely in the dark. The information accumulated by the analyst respecting the composition of the ores, fuel and fluxes available at the works, and the composition of the pig-iron and slags or cinders, produced under varied conditions, in regard to materials employed, and to the proportion of ore, fuel, and flux used in the blast furnace, could not, however, exist long without exerting a marked beneficial influence upon the quality of iron produced, and generally upon the iron industry of the country.

Percy's invaluable work of reference on Metallurgy furnishes abundant evidence of the scientifically interesting, as well as practically useful, nature of the results obtained at that time by the chemists above named, and others, working under Dr. Percy, with respect both to the elaboration of important analytical processes (in which direction Mr. Riley has continued to the present day to do valuable work) and to the elucidation of the reactions occurring in the processes of reduction and refining of the metal. It is needless to dwell upon the fact that the aid of the analyst has now long since become absolutely indispensable to the iron and steel manufacturer; but I may, perhaps, be allowed briefly to refer to one or two recent illustrations of the indispensable part which analytical research has played, and continues to play, in the extension of our knowledge of the chemical reactions involved in the production of cast and wrought iron and of steel, and of the influences which the chief associates of iron in its mercantile forms exert upon its physical characters.

Among the many valuable communications made to that most important body, the Iron and Steel Institute of Great Britain, by men who combine great practical knowledge and experience in iron and steel manufacture with high attainments in mechanical science and such knowledge of chemical science as insures a full appreciation of its value at their hands, one of the most interesting and suggestive to the chemist is that on the separation of carbon, sulphur, silicon, and phosphorus in the refining and puddling furnace and in the Bessemer Converter, contributed to the *Transactions* of the Institute's recent meeting, by Mr. Lowthian Bell, whose valuable investigations in connection with the iron industry are as interesting to the chemist as they are useful to the manufacturer. Mr. Bell has brought together the results of an extensive series of practical experiments on the treatment of different kinds of pig-iron of known composition, in the finery, the puddling-furnace, and the Bessemer Converter, and, by comparing the results of analytical investigation of the products of those experimental operations with each other and with those of the materials operated upon, he has obtained valuable confirmation of the views already held by metallurgic chemists regarding the succession in which carbon, silicon, sulphur, and phosphorus are attacked when pig-metal is submitted to the above purifying processes, and the extent to which those foreign associates of iron are abstracted or resist removal, by the more or less thorough application of those several modes of treatment. He has also thrown new light on the reasons why the most difficultly-available

impurity, phosphorus, obstinately resists all attempts to effect even a slight diminution in its amount by application of the Bessemer treatment. The earnestness with which Mr. Bell wages war against this enemy of the iron-master in one of its most favorite haunts, the Cleveland District, not simply with the old British pluck, which acknowledges not defeat, but systematically, on scientific principles, calling to his aid all the resources which the continual advances in applied mechanical and chemical research place within his reach, cannot fail to contribute importantly, if it does not of itself directly lead, to the complete subjection of this most untractable of the associates to which iron becomes linked in the blast-furnace. Indications have lately not been wanting that the existence of phosphorus in very notable proportion in iron may not of necessity be inimical to its conversion into steel of good quality, and it may be that this element, which is now turned to useful account to impart particular characteristics to the alloys of copper and tin, is even destined to play a distinctly useful part in connection with the production of steel possessed of particular characters valuable for some special purpose.

In the great development which steel manufacture has received within the last few years, one most prominent feature has been the production, with precision, upon a large scale, of steel of desired characteristics, in regard to hardness, &c., by first adding to fluid cast-iron of known composition the requisite proportion of a rich iron ore (with or without the addition of scrap iron) to effect a reduction of the carbon to the desired amount, concurrent with a refining of the metal by the oxidising action of the ore, and then giving to the resulting steel the desired special qualities by the addition of suitable proportions of iron compound of known composition, rich in manganese and carbon (Spiegeleisen and the similar product called ferro-manganese). The germ of this system of producing steel varieties of predetermined characteristics exists in crucible processes like that of Uchatius, which have been in more or less extensive use for many years past, but it is to such invaluable arrangements as are most prominently represented by the Siemens-Martin Furnace—wherein several tons of metal may be fused and maintained at a very high temperature with as little liability to change from causes not under control, as if the operation were conducted in a crucible—that we are indebted for the very great expansion which the direct application of the analytical chemist's labors to the development of the steel industry is now receiving.

The production of steel upon the open hearth, to the elaboration of which Dr. C. K. Siemens has so largely contributed since he first established the process at Llandore in 1868, has in fact, become assimilated in simplicity of character and precision of results to a laboratory operation, and may be justly regarded as a triumph of the successful application of chemical principles and of the power of guidance and control afforded by utilising analytical research, to the attainment of prescribed results upon a stupendous scale, with an accuracy approaching that which the experienced chemical operator secures in the laboratory upon a small scale, under conditions which he can completely control. The production of steel by a large number of small separate operations in pots has now become supplanted with great advantage by the Siemens-Martin system of working at some of our largest establishments at Sheffield; this system has also secured a footing at highly renowned Continental works, which are formidable competitors with us in the manufacture of steel, such as those of Essen, Creusot, and Terrenoire. It is specially interesting to notice that, in the hands of those who, on the Continent at least equally with ourselves, have learned to combine the results of practical experience with the teachings of chemical science, the facilities now existing for dealing in a single receptacle with large masses of fluid steel have greatly facilitated the application of chemical means to the production of *solid* masses of considerable size, thereby reducing, if not altogether dispensing with the necessity for submitting large steel castings to costly mechanical operations with the object of closing up cavities caused by the escape of occluded gas as the liquid metal cools. The success in this direction which appears to have attended the addition of silicon, in com-

bination with iron and manganese, to the steel before casting in preventing the formation of so-called *blow-holes*, and in contributing at the same time to the production of the particular character of steel required, bids fair to be of special importance in connection with the application of steel to the production of projectiles for use against armor-plates, as affording ready and comparatively very economical means of ensuring the production of perfectly sound castings, or which in compactness of structure will, it is asserted, compete successfully with carefully forged castings, and even with the magnificent material which Whitworth produces by submitting the fluid metal to powerful pressure.

The part which silicon plays by its comparatively high susceptibility to oxidation, in promoting the production of sound steel castings is readily inteligible, but the functions of the manganese compounds which are an indispensable adjunct to the *Bessemer* process, and the application of which has become an integral part of steel manufacture, are still far from being thoroughly understood, and there is ample scope for chemical research, in co-operation with practical experiment, in the further study of the influence not only of manganese in the production, and upon the properties of steel, but also of elements such as titanium, tungsten, and boron, and of chromium, which exists, associated in considerable quantities with iron, in a very abundant Tasmanian ore, to which prominent attention has lately been directed. The achievements of the mechanical engineer have so facilitated the handling and perfected the means of production and the mechanical treatment of malleable iron and of steel, that the full advantage may now be reaped of any improvement of a chemical nature which may be effected in the production of those materials; and it must be a source of pride to the chemist to observe with what success the teachings of his science are being applied, by practical men of the present day, in the construction of furnaces capable of withstanding the high temperatures required for the production and working of iron and steel in large masses, and in combining the perfect consumption and consequent great economy of fuel with the attainment of those high temperatures and with a thorough control over the character of the gaseous agents to which the fluid metal is exposed in the furnace. I need not quote the names of those men who have already rendered themselves prominent by their services in this particular direction, but may refer, in special illustration of the results achieved by purely practical men, to the success in applying very simple furnace arrangements to the attainment of the above results which has recently attended the labors of Mr. William Price, a principal foreman in the Royal Gun Factories at Woolwich.

A few experiments made in the early days of the application of armoring to ships and forts appeared to demonstrate on the one hand that steel was quite incapable of competing with malleable iron of even very moderate quality as a material for armor-plates, and, on the other hand, that the penetrative power of projectiles made of chilled iron upon the Palliser system could not be surpassed or even attained, with any degree of certainty, by projectiles of steel produced at comparatively very great cost. But some recent results obtained on the Continent, and especially in the course of the important experiments instituted by the Italian Government at Spezzia, have afforded decisive indications that steel, the application of which to the construction of ordnance has since that time been very greatly extended, may now be looked to hopefully as capable of affording greater protection against the enormous projectiles of the present day than can be secured by proportionately large additions to the stupendous iron-armoring of the most modern ironclads, and also as applicable at a cost very moderate, when compared with that of ten years ago, to the production of projectiles of large dimensions superior in point of penetrative power and of uniformity in this respect to those of chilled iron, the difficulties in the production of which are very greatly increased by the formidable increase which has lately been made in their size. Promising results have also quite recently been obtained at Shoeburyness with a new system of applying steel in conjunction with malleable iron, by which a perfect union of the two materials at one of

their surfaces is effected by the aid of heat.

The superiority of soft and very homogeneous steel over wrought iron of the best quality in regard to lightness, combined with strength and toughness, are leading to its very advantageous employment in the construction of a particular class of vessels for the navy; and the perfect confidence which can be placed in the uniformity in structure and strength of steel of such character as is produced by the Whitworth system of manufacture has greatly facilitated the production of air-chambers of small weight, but capable of being quite safely charged with sufficient air, under a pressure of 1,000 pounds on the square inch, to carry the Whitehead torpedo through water to a distance of 1,000 yards in little more than a minute and a half.

Thus, the results of the recent development of steel-industry, to which the labors of the chemist have not unimportantly contributed, give promise of exerting a great influence upon the resources of nations for defense and attack. Although the necessity for the continual expansion of such resources cannot but be deeply deplored, there can be no doubt that the problems which it presents, and the special requirements to which it gives rise, must operate, and perhaps as importantly as the demands created by peaceful industries and commercial enterprize, in encouraging the metallurgist, the chemist, and the engineer to continue their combined work in following up the successess, to the achievement of which the results of scientific research have greatly, though indirectly, contributed.

If it were necessary to add to the illustrations which Mr. Perkin gave in his address last year of the practical fruits of research in *organic* chemistry, I might be tempted to dilate upon the important results which have, especially during the last ten years, grown out of the discovery and study of the products of the action of nitric acid upon cellulose and glycerin. During the six years which have elapsed since I had the honor of bringing before the members of the British Association the chief points of scientific interest and practical importance presented by the history of those remarkable bodies, their application to technical and war purposes has been greatly developed. Nitro-glycerin and gun-cotton may now be justly classed among the most interesting examples of the practical importance frequently attained by the results of chemical research, while the history of the successive steps by which their safe manipulation and efficient application have been developed affords more than one striking illustration of the achievements effected by combined physical and chemical research in the solution of problems of high scientific interest and practical importance, and in the vanquishment of difficulties so formidable as for a time to appear fatal to the attainment of permanently practical success.

It is to a careful study of the influence which the *physical* character of gunpowder (its density, hardness, &c.) and its *mechanical* condition (*i.e.*, form and size of the masses and condition of their surfaces) exert upon the rapidity of its explosion under confinement, that we chiefly owe the very important advance which has been made of late years in controlling its explosive force; in its applications as a propelling agent, and the consequent simple and effectual means whereby the violence of action of the enormous charges now used in siege and ship-guns is effectually reduced to within their limits of endurance without diminution of the total explosive force developed. But, concurrently with these important practical results, the application of combined chemical and physical research to a very extended and comprehensive investigation of the action of fired gunpowder has furnished results which possess considerable interest from a purely scientific point of view, as in many respects modifying, in others supplementing, the conclusions based upon earlier experiments and theoretical considerations with respect to the nature and proportions of the products formed, the heat developed by the explosion, the tension of the products of combustion with the conditions which regulate it both when the explosion is brought about in a close vessel and when it occurs in the bore of a gun. The results of these physico-chemical researches have, moreover, already acquired practical importance in regard to the light they

have thrown upon the influence exerted by variable conditions of a mechanical nature upon the action of and pressure developed by fired gunpowder in the bore of a gun, and in demonstrating that modifications in the *composition* of gunpowder, not unimportant from an economical point of view in dealing with the very large charges now employed, may importantly contribute to render the storing of the maximum of work in the projectile, when propelled from a gun, compatible with a subjection of the gun to comparatively very moderate and uniform strains.

Other interesting illustrations of the intimate manner in which physical and chemical research are linked together, and of the important extent to which some of our most illustrious workers in chemistry have contributed to demolish the semblance of a barrier which existed in past times between the two branches of science, are furnished and suggested by the recently published List of Grants of Money which the Government has made to scientific men, on the recommendation of the Royal Society, from the fund which, for the first time last year, was added to the very modest sum previously accorded from national resources in support of research. The perusal of that list, representing as it does a most carefully considered selection by the highest representatives of science in the country, from a very large number of applications, affords important evidence, on the one hand, of the active pursuit of science in Great Britain, and, on the other, of the very wide range of subjects of interest and importance, the full investigation of which demands the provision of adequate resources. That the necessity for such resources needs but to be thoroughly made known to ensure their provision, even from other than national sources, has been demonstrated by the success which, in a comparatively brief space of time, has attended the efforts of the Chemical Society to establish, upon the foundation patriotically laid by one of its original members, Dr. Longstaff, a special fund, to be administered by the Society for the advancement of chemical science. An inspection of the list of contributors to this special fund in aid of chemical research which, in about two years, has reached the sum of four thousand pounds, and from the proceeds of which the first applications for grants have recently been met, is suggestive of two observations. One is, that the proportion and amount of contributions hitherto received are comparatively small from the source whence the greatest support of such a fund may naturally be looked for, namely, from those who most directly benefit by the results of chemical research. It is to be hoped that there are many prominent representatives of the chemical and metallurgic industries in this country who still intend to give practical effect to their natural desire to aid in the advancement of chemical science, and to the appreciation which they can hardly fail to entertain of the usefulness of this fund. On the other hand, it is a matter well meriting special notice that a very prominent section of the contributors to the fund is composed of some of the most ancient corporate bodies of the city of London. Most welcome evidence is thereby afforded of the readiness with which the City Companies are prepared to respond to appeals for the substantial support of measures well calculated to promote progress in science. This evidence, and the combined action which they are even now contemplating for promoting the application of scientific research to the advancement of industry and commerce, by establishing an institution for technical education upon a scale worthy to serve as a monument of the true usefulness of wealthy confederations, must be cordially hailed as very substantial proofs that these representatives of our national wealth and commercial supremacy are entering upon a new sphere of activity which will more than restore their ancient prestige, by according them a new rank, more elevated than any which their civic importance could, in the past or future, confer upon them—a rank high among the chief promoters of our national enlightenment.

GUNBOATS built for the Chinese Government have proved excellent sea boats in their outward voyages. Lord Napier of Magdala, who inspected them at Gibraltar, expressed a very high opinion of their qualities and powers of river defence and aggression.

THE BREMEN WATERWORKS.

By E. BOTCHER and H. OHNESORGE.

From "Zeitschrift fur Bauwesen."

The following were the chief points laid down, before this work was commenced, by Herr Berg, chief architect to the town. As the number of inhabitants was about seventy thousand, provision should be made for a supply of 900,000 gallons per diem. A reserve machine should, however, be provided, capable of pumping another equal quantity; and in laying the pipes, provision should be made for an increase in the supply, and for an eventual doubling. The level of the highest inhabited part of the town is 39 feet above Bremen datum.

It was at first proposed to procure the supply from the ground by natural filtration, but experiments showed that such water could not be used. It was found that the river was perfectly free from metallic salts in solution, but that after filtration through the clay bed sulphuret of iron (Schwefeleisen) appeared in appreciable quantities. It was therefore determined to obtain the water from the river, and to purify it by artificial filtration.

The works are situated on a small peninsula, formed by the junction of the great and small Weser. They consist of a handsome square building containing the pumping machinery, and two large water-tanks placed 128 feet above datum. The building has four towers, which are respectively appropriated for the chimney-shaft, the falling and rising mains, and the staircase. On the side of this building is the boiler-house, and on the other corresponding to it a dwelling. Behind these is a large covered reservoir and filtering basins. The water is obtained from the Great Weser, and is admitted by a pipe into a well, from which it is pumped into the filtering basins; thence it passes into the covered reservoir, and is pumped into the tanks and into the pipes.

The filtering basins have each an area of 808 square yards. The sides and bottom are of bricks set in trass mortar, and plastered over with Portland cement mortar. The bottom bend is 9.17 feet above datum. The following is the arrangement of the filter layers and their respective thicknesses, beginning from the bottom:

a.	Coarse shingle in thickness of		20.87	inches.
b.	Middling	"	7.56	"
c.	Fine	"	5.71	"
d.	Coarse gravel	"	5.71	"
e.	Fine	"	5.71	"
f.	Fine sand, washed	"	28.47	"

The top level of the sand layer is, therefore, 15.30 feet above datum.

Experience has shown that the deposited matter does not infiltrate beyond 1 inch, and it then forms such a dense crust that the filtering basin cannot be further used until it is removed. This is easily done by sod-cutters, and can be repeated for some time before the sand need be replaced. The pipe by which the water is admitted is 14.85 inches in diameter; its position being vertical, with a trumpet-shaped mouth. The water, having passed through the filters, is carried off by nine small channels into one larger, whence it passes into the covered reservoir through a stand-pipe having two outlets; one level with the bottom, and the other about 11 inches below the ordinary water-level of the filtering basins. When filtration is proceeding rapidly, the latter is used; when slowly, the former, by which the rate can be kept nearly uniform.

The covered reservoir has an area of 1,600 square yards, exclusive of the pillar space. The bottom, which is 7.4 feet above datum, is covered with a layer of Portland cement mortar, 1 inch thick. To protect the water in the reservoir from frost it is arched over between iron girders, and these arches are covered with a layer of clay 3 feet thick. The main building had to be erected on piles, and its size was dependent on the two tanks placed 128 feet above datum.

The sizes of the pipes in use vary from 20 to 3 inches. They are socket pipes, and their lengths and dimensions are given in detail. Hydrants for a 2½-inch hose are placed about 100 yards apart. One hundred and twenty fountains have

also been provided for free use. The pipes were tested to 15 atmospheres; and at 12 atmospheres they were rapped with hammers varying in weight from ½ lb. to 3 lbs. The water has to be taken over two bridges; and from the construction of the largest one of them, the pipe of 19.8 inches diameter is split into three small ones of 13.88 inches diameter, one of them being a reserve pipe. It also passes underneath the timber harbor for 45 yards through wrought-iron pipes. The laying was done by Messrs. J. and A. Aird, between November 1872 and April 1873.

The five Cornish boilers are usually fed by the donkey in the engine-house, but have also an independent steam-pump. The steam-chest is placed across the end of the boilers, and the steam-pipe goes along the top of the flue into the engine-house. The chimney-shaft is 3.78 feet in diameter, and is constructed of fire-brick masonry to 39 feet above datum, from whence the remaining 136 feet are formed by a cast-iron pipe.

The two double-cylinder horizontal steam-engines are exactly similar. The diameter of each cylinder is 19.5 inches, and the stroke 58.5 inches. The piston-rods of each are prolonged, the one working a so-called filter-pump, the other a high-pressure pump. Each engine has its condenser, and works an air-pump, a feed-pump for the boilers, and a small air-pump for the air-pressure in the mains.

The filter pump has a diameter of 18 inches, and a stroke of 58.5 inches. It lifts the water from the well in communication with the Weser; draws it through two air vessels, from whence it flows by a pipe 20.82 inches in diameter, and common to both engines, into the filtering basins. The lift is usually about 15 feet, and the quantity discharged per minute 1,540 gallons.

The high-pressure pump has a diameter of 17.86 inches, and a stroke of 58.5 inches. It lifts the filtered water out of the covered reservoir in a similar manner through a 14.85 inch pipe, and into the rising main, and thus into the tanks above. The lift is usually about 120 feet; and the quantity raised per minute 1,500 gallons.

Details are given of the various steam and pipe arrangements. The rising main is 20.82 inches in diameter, and the falling main 19.81 inches. They are both constructed of flange pipes, and have the necessary compensating arrangement under the tanks. The engine room contains self-registering gauges of the water-level in the five filtering basins and the reservoir, and a bell which rings as soon as the tanks are full. There are self-registering guages of the water-level in the tanks and in the well, as well as a manometer giving the pressure in the rising main.

Experiments were made to test the guaranteed coal consumption with very satisfactory results. The quantity of coal required to raise 220 gallons 120 feet being 0.776 lbs. (34,000,000 of duty).

The size of the tanks is 73 feet by 35 feet, by 11 feet high, and they are able to contain 372,500 gallons of water each. They are constructed of iron plates, with angle-iron ribs 6.4 feet apart. The total weight of each is about 80 tons. Each rests on nineteen cross girders, constructed to carry 3.7 tons. These rest on fourteen principal girders, with a span of 40 feet. The width of one of these girders in the center is, in proportion to its length, as 1 to 8. Its lower side forms a parabolic curve, joined to the upper horizontal side by seven uprights, which are joined to each other by diagonals. They rest at each end on cast-iron blocks which work on rollers, and so allow for the consequences of change of temperature. The total weight of all the girders is about 180 tons.

The cost of the work was £152,250.

A City Railroad is now under construction in Berlin. It is to extend from the Lower Silesian depot to Charlottenburg, and will be seven miles long, and is expected to cost $1,071,000 per mile, and as half the right of way and grounds is already acquired and the work has been in progress a year, the estimate should not be very far out of the way. The road is to have four tracks, two for the through traffic of the roads with which it connects, which are six in number. Each of these lines is to have three stations in the city. The other two tracks will serve exclusively for local traffic, and will have six stations varying from less than ½ to 2¼ miles, and probably two other stopping places will be provided.

THE MECHANICS OF VENTILATION.

By GEO. W. RAFTER, C. E.

Written for VAN NOSTRAND'S MAGAZINE.

II.

The velocity of 7.3 lineal feet per second will therefore be in the pipe under the register, and the discussion may be continued on the supposition that the register is absent by using a sufficiently large value for the coefficient of influx in the expression for head following.

Then if resistance from friction and elbows and all other sources of resistance were entirely eliminated the head necessary to produce a velocity of 7.3 lineal feet per second throughout the small ducts would be sufficient to exhaust all the impure air. In fact, the reduction for friction, elbows, etc., gives us widely varying velocities for different parts of the ducts, and requires in every case an addition to the head.

To find head $=h'$ we take formula (16) and using the following values :

$$\xi_1 = 0.840,$$
$$\xi_0 = 1.750,$$
$$\xi = 0.0205,$$
$$l = 36,$$
$$a = 6'' = 0.5 \text{ feet},$$
$$b = 8'' = 0.66 \text{ feet},$$
$$V^2 = (7.3)^2 = 53.29,$$
$$2g = 64.4,$$

we have

$$h' = \left\{ 1 + 0.840 + 1.750 + \left(0.0205 \frac{36(0.5+0.66)}{2(0.5\times 0.66)} \right) \right\} \frac{53.29}{64.4}, \quad \therefore$$

$$h' = 4.04$$

Applying similar reasoning to the main ducts in basement and complete expression for h' aside from the vertical exhaust flue is found as follows :

$$h' = 4.71 \text{ feet.}$$

According to formulæ (1) and (2) illustrating Gay Lussac's law for constant pressure, the volume of one and the same quantity of air increases or decreases directly as the temperature.

The average temperature of the air in the school-rooms was found to be 68°, while the temperature at the foot of the main shaft was found to be from 52° to 56°, average 54°. The air had lost then in passing to that point 14°. This reduction of temperature mainly took place in the main ducts in basement, the volumes of which equal say 1500 cubic feet.

Suppose 1500 cubic feet of air at 68° to pass into the main ducts in basement, and there become cooled to 54°, a reduction of volume takes place found by formula (1) thus :

$$V_0 = 1500 \text{ cubic feet}$$
$$t = 54°$$

In place of 32° in the formula substituting 68° the temperature at which $V_0 = 1500$, and, because the volume at a reduced temperature is required, changing the signs of t and 68° and changing the sign of a, we have for value of V at 54°

$$V = 1 - 0.00204(68-54)\, 1500 = 0.97144 \times 1500, \quad \therefore$$

$$V = 1457 \text{ cubic feet},$$

and $1500 - 1457 = 43$ cubic feet.

We see, therefore, that a vacuum is continually forming in the main ducts and adding materially to the exhausting capacity of the works. For this reason the ventilation will probably work more freely in winter than in summer, unless means are provided to raise the temperature in main exhaust flue in a corresponding ratio during the latter season. Taking formula (15)

$$h' = \frac{ha(t'-t)}{1+at}$$

it is required to find the value of t', which will produce h' as found by previous calculation. Substituting numerical values as follows :

$$h' = 4.71,$$
$$h = 55,$$
$$t = 54,$$
$$a = 0.00204,$$

and we have—

$$4.71=\frac{55\times 0.00204\,(t'-54)}{1+0.00204\times 54},\ \therefore$$

$$4.71=\frac{0.1122\,t'-6.058}{1.11},\qquad \therefore$$

$$5.228=0.1122\,t'-6.058,\quad \therefore$$

$$0.1122\,t'=11.286,\qquad \therefore$$

$$t'=101^\circ.$$

101° is then the constant temperature at which the air in vertical shaft is to be kept in order to produce the action through the ducts of a head equal to 4.71 feet.

It should be remembered that this calculation involves the supposition that the pressure is that due to the air at 54°. This supposition is only partly true, as the outside temperature will at different times vary widely from that point. The instability of equilibrium will extend itself not only to the air in the ducts and building, but also to every particle of outside air. The actual effect will depend upon a variety of circumstances, as, for instance, the constancy of supply of outside air, the method of introducing it, the temperature at which the outside air is delivered, the closeness of fitting of doors and windows and the tightness of the ducts, &c. For summer ventilation an increase of heat over that necessary for winter ventilation will be required. The capacity of the heating apparatus at foot of main shaft should, therefore, be in excess of winter requirements.

We have seen that in fact the original volume of 850,000 cubic feet per hour must be considerably less than that amount when reaching the foot of the main shaft, by reason of reduction of temperature. In order to make adequate provision for summer ventilation we will continue the discussion on the supposition that we have to deal with a volume of 850,000 cubic feet at foot of main shaft.

To raise 850,000 cubic feet from 54° to 101° will increase its volume to 931,498 cubic feet. Discharge per second then, equals

$$\frac{931498}{3600}=259 \text{ cubic feet.}$$

h' as found above equals 4.71 feet, also

$$h'=\frac{ha(t'-t)}{1+at}$$

therefore substituting in formula (13), for the present neglecting E, and we have

$$V=\sqrt{64.4\times 4.71}=\sqrt{303.324},\quad \therefore$$

$$V=17.41 \text{ lineal feet.}$$

Since E was neglected we have 17.41 lineal feet as the theoretical velocity which will produce the necessary exhaust. Experiment indicates a reduction of the theoretical velocity in this flue of .12 for friction and eddies, consequently the actual velocity with a head of 4.71 feet, or what is the same with the flue heated to a point to produce that head, will be $0.88\times 17.41=15.32$ lineal feet. But a velocity of 15.31 feet in the vertical flue corresponds to a reduction of velocity in the ducts, we must therefore, increase h', and consequently t' by an amount sufficient to produce an actual velocity of 17.41 lineal feet per second. To calculate that increase we have 17.41 equals required actual velocity, consequently, corresponding theoretical velocity will be

$$\frac{17.41}{0.88}=19.78.$$

The value of h' which will produce a theoretical velocity of 19.78 lineal feet will be found by taking—

$$19.78=\sqrt{64.4\times h'}.$$

Squaring this equation—

$$391.24=64.4\times h',\ \therefore$$

$$h'=\frac{391.24}{64.4}=6.07 \text{ feet.}$$

It has been shown by what has preceded that the value of t' depends upon the value of h'. It will be necessary then, to find a new value of t' to correspond with the value of $h'=6.07$. This will be found as in the preceding case by substituting in formula (15), whence we have for ultimate value

$$t'=114^\circ.$$

To prove the correctness of these calculations we substitute complete values in formula (13), giving

$$V=0.88\sqrt{64.4\,\frac{55\times 0.00204(114-54)}{1+0.00204\times 54}},$$

whence by reduction

V=17.41 lineal feet as previously found.

Translated from algebraic into common language, the preceding discussion means that a confined column of air at a temperature of 54° and original height of 55 feet, when heated to a temperature of 114° expands to a height of (55′+6′.07)=61.07 feet. But the height of the flue remains constant at 55 feet. The heated confined column then, when issuing from the flue becomes immediately cooled. The 55 feet of air in the flue is lighter than the outside air, by the weight of a column of the density of the confined column and 6.07 feet in length. According to the principle of equilibrium of fluids, the confined column must rise 6.07 feet in seeking such equilibrium, or, what amounts to the same thing, the outside air falls by approximately that amount.

We are now able to say what sectional area of shaft is required. By transposition of general formula (17) for velocity we have

$$F=\frac{Q}{V},$$

that is to say, the sectional area equals the quantity per unit of time, divided by the velocity per unit of time, therefore

$$F=\frac{259}{17.41}=14.9 \text{ square feet.}$$

As already stated the shaft is constructed with cross-section equal to 26.62 square feet.

Attention may here again be called to the fact that theoretically, velocity in the shaft is entirely independent of the cross-section, depending as previously shown, upon temperature, height and adaptation. In practice, however, it will be found of the greatest importance not to have the shaft larger than careful calculation shows necessary. Indeed the case of a confined column in motion by excess of temperature is one where it is absolutely better to have the shaft smaller than calculation shows, rather than larger. If the flue has less sectional area the required discharge may still be produced by increasing the heat, so attaining higher velocities. If, however, the sectional area is greater than really necessary, the following condition of affairs results: The current of hot air passes up along the axis of the flue, while at the sides a current of cold air passes down. The result is the production of eddies and unnecessary cooling of the confined column.

An experiment at No. 9 aptly illustrates the point in question. Temperature of air entering the shaft 52°; temperature eight feet above fire 120°. The air had then been raised through 68°. This would be ample were the shaft of proper size to produce the necessary exhaust. A piece of tissue paper thrown into the middle of the ascending current and remaining there was carried to within ten feet of top of shaft in three seconds. The motion was however very irregular. When at the above point the slip was caught in an eddy and brought back to foot of shaft, was there secured and again sent up. The second time it maintained its position in the middle of the flue and passed out at the top in a little more than four and one half seconds. Repetition of this experiment under many different conditions showed that with a difference of temperature of 50° at the foot of the shaft the lightest tissue paper would rise less than one half the length of the shaft, while with a difference of from 30° to 40°, 10 to 15 feet was the extent of its rise.

We have in these experiments a significant explanation of the cause of smoky chimneys. A chimney has usually a certain specified work to perform, and like every other contrivance for the performance of work will obey the law of adaptation. In short, even in the construction of apparently so simple a thing as a chimney, it is unsafe to set at nought nature's universal laws.

We see farther that the expedient of constructing everything a little larger than necessary, in order to have a reserve for contingencies, is not always a safe one after all.

The following criticisms can be justly made of the No. 9 ventilating arrangements as they at present stand.

1. The vertical shaft has too great a sectional area.

2. The square elbows add greatly to the resistance. An increase of power is thereby necessitated to perform the work.

3. The ducts leading from the rooms

are too small, for two reasons: first, to carry away the maximum amount with their present cross-section requires so high a velocity of influx as to be uncomfortable to the occupants; second, friction is greater in small pipes than in large ones, as may be shown by the following: The height of resistance of friction, due to dimensions of pipe and to velocity of discharge, in a round pipe is expressed by formula thus:

$$h_0=\xi\times\frac{1}{2g}\times\left(\frac{4}{\pi}\right)^2\times\frac{lQ^2}{d^5}$$

where ξ=the coefficient of friction determined by experiment, l=length of pipe, d=diameter and Q=discharge.

Let h_1=the height of resistance of friction, &c., for a pipe of length also equal to l and diameter equal to $2d$, the discharge to be Q as above, then,

$$h_1=\xi\times\frac{1}{2g}\times\left(\frac{4}{\pi}\right)^2\times\frac{lQ^2}{(2d)^5}.$$

dividing

$$\frac{h_0}{h_1}=\frac{\xi\times\frac{1}{2g}\times\left(\frac{4}{\pi}\right)^2\times\frac{lQ^2}{d^5}}{\xi\times\frac{1}{2g}\times\left(\frac{4}{\pi}\right)^2\times\frac{lQ^2}{(2d)^5}};$$

whence by reduction

$$\frac{h_0}{h_1}=\frac{32}{1},\ \therefore$$

$$h_0 : h_1 :: 32 : 1,$$

that is, the friction in the small pipe is 32 times what it is in a pipe of double the diameter, the discharge being constant. In the same way it can be shown that with a diameter three times as great the friction would be as 3^5 to 1 or as 243 to 1 and so on.

4. The exhaust openings into the rooms are directly in the floor; the pipes, therefore, soon become filled with dust and papers.

5. The small ducts leading to the rooms are all of a size, whereas, they should increase according to the law of friction as distance from the vertical flue increases.

6. The form of shaft is faulty. It should be smaller at the top than at the bottom to compensate for the inevitable shrinkage of volume of the air current, resulting from loss of heat by radiation.

We will now determine the amount of coal which when burned will produce the necessary head.

A pound of coal contains on an average 14,500 pound-degrees or heat-units, that is, the heat from the combustion of one pound of coal will raise 14,500 pounds of water 1°, or, what is the same will raise one pound of water 14,500°. Specific heat of air is 0.238 when water is unity; or more clearly, to heat a pound of air from any temperature a, through a given number of degrees b to a temperature $a+b$, will require only 0.238 as much heat as would be necessary to raise a pound of water through the same number of degrees.

As already noted the air arrives at the point of heating—the foot of the shaft—at a temperature of 54°. It is required to raise it to 114°, or through 60°. To raise one pound of air 60° we have $1\times60\times0.238=14.28$ heat-units. Farther a pound of air at 32° equals 12.34 cubic feet, therefore by formula (1) we have for volume at 54°

$$V=[1+0.00204(54-32)]12.34,\ \therefore$$

$$V=12.89 \text{ cubic feet.}$$

850000 cubic feet of air at temperature 54° will equal

$$\frac{850000}{12.89}=65865 \text{ pounds.}$$

The number of pound-degrees required per hour will be

$$65865\times14.28=940552.$$

The number pounds of coal required for seven hours, the time ventilation is required each day, will be

$$7\left(\frac{940552}{14500}\right)=454 \text{ pounds.}$$

There is, however, a certain loss of heat by conduction through the walls of the flue, determined by the formula

$$Q=ka\frac{t'-t}{d} \qquad (18)$$

in which the notation is as follows:

Q=quantity of heat conducted away per hour.

k=coefficient of conduction=0.085*.

a=area of interior surface of the flue =1135 square feet,

* For values of k for various materials, see volume 2, Weisbach's Mechanics.

t'=inside temperature=114°,

t=outside temperature, taken at 32°,

d=thickness of wall=0.35 feet.

Substituting numerical values and making the expression for loss during seven hours, gives,

$$Q=7(0.085\times1135)\frac{114-32}{0.35},\therefore$$

$$Q=158228 \text{ heat-units},$$

requiring a consumption of coal to make good this loss of

$$\frac{158228}{14500}=10.8 \text{ pounds}.$$

Adding this to the above and we have 465 pounds of coal=total consumption each day.

The question naturally arises how shall this heat be applied? Obviously there is no loss of heat except as just indicated by conduction. The heat may be applied then, by large grate or by furnace, with ample opening for draught, and with smoke-pipe of large diameter extending to within 15 feet of top of flue.

One point in particular brought out by studying this system of ventilation is worthy of careful attention, namely: To ventilate a building means there is work to be done. The performance of work requires an expenditure of force, therefore, any system professing to secure ventilation without performing work, or what is the same, without expending force, is without effect.

It is not denied that natural ventilation may at times prove efficient, though it is so uncertain in its action that in buildings of any size it is practically useless.

The ventilation of a building should be like its water supply and sewerage, always ready for work and certain in its effects. When buildings are erecting such a condition can be attained at moderate expense, while it may be exceedingly difficult to accomplish after the completion. It is, then, of the highest importance that ventilation be considered in connection with design.

The warming at No. 9, is by stoves in each room. A cylinder of galvanized iron is placed about the stoves, thus converting them into hot air furnaces, the air in which may be taken as a heated confined column. Fresh air is introduced at floor from outside, by ducts of sectional area $(12''\times16'')=192$ square inches, and from 2 to 3 feet in length. Average temperature of air in cylinder 220°, outside temperature taken at 32°. Cylinders from 6 to 8 feet in height. Velocity of inflow is found by formula (13). Amount of inflow under the most favorable conditions equals 21.5 cubic feet per second, giving a supply of 67,400 cubic feet per hour for each room.

This is a cheap way of improving the warming of a building already furnished with stoves. The diameter of the cylinders should be at least 2 feet greater than that of the inclosed stove, and their length should be nearly equal to the height of the room. The inflow duct should be of ample cross-section and provided with a slide in order that the supply may be regulated. The supply in any case should be sufficient to prevent the temperature of the issuing air rising much above 120°.

The amount of air required to support combustion may be calculated as follows: Let the amount of air to be supplied to any room be 65,000 cubic feet per hour or 455,000 cubic feet for seven hours, or in the same proportion for any number of hours, then

$$\frac{455000}{13.0}=35000=\text{number of pounds to}$$

be supplied for seven hours at temperature of 60°.

We will take outside temperature at 32°, and for purposes of this calculation, in order to make an allowance for loss by radiation, we will take the temperature to which the air is to be raised at 75°. This would involve raising the temperature through 43°. The number of heat-units required per day of seven hours, therefore, will be

$$35000\times43\times0.238=358190.$$

The daily consumption of coal under the above conditions is

$$\frac{358190}{14500}=24.7 \text{ pounds}.$$

Observation and calculation have demonstrated that the amount of air required to consume a pound of coal is on an average 150 cubic feet. From imperfection in burning appliances, etc., the consumption frequently rises above twice

that amount, and 300 cubic feet to the pound is generally used for these calculations. We have for the consumption during seven hours

$$24.7 \times 300 = 7410 \text{ cubic feet.}$$

In applying this matter practically it must be borne in mind that usually *larger quantities* will be needed than calculation indicates unless very liberal allowances for loss by radiation and conduction are made.

A new phase of ventilation has recently been developed by a German philosopher, Pettenkofer by name, who found that by diffusion alone, the air of a room in his house (cubic contents 2650 feet) was changed once every hour when the difference of exterior and interior temperatures was 34°. With the same difference of temperature, but with the addition of a good fire in the stove, the change rose to 3320 cubic feet per hour. With all the crevices and openings about doors and windows pasted up air tight the change amounted to 1060 cubic feet per hour. With a difference of 40° between exterior and interior temperatures the ventilation through the walls amounted to 7 cubic feet per hour for each square yard of wall surface. The practical deduction from which is, that ventilation may be much improved by merely increasing the amount of free wall surface. In designing school houses, particularly, such an arrangement should be adopted as will insure at least two outside exposures, thus giving not only better ventilation, but also rendering it certain that a sufficient supply of light be admitted.*

One way of applying the vacuum method of ventilation is to carry the smoke flue from furnace in basement up through a vertical shaft from which openings into the rooms are made. Several examples of this form of construction recently examined by the writer have the smoke flue of vitrified sewer pipe. This offers a cheap method of forming the flue, though it can scarcely be recommended on account of the low conducting power of the material, the conducting power of fire-clay, the main material of vitrified pipe, being 39.6 less than that of wrought iron.

* Hartley's Lectures. "Air and its relation to life."

An important application of the formulæ for confined columns, etc., may be made in designing the flues for hot air in buildings warmed by furnaces. The method quite frequently followed at present is to make the flues leading to the various stories of a building all of the same size, and this without reference to the horizontal distance from the source of supply. The result of this unscientific proceeding is that down currents are created in the pipes leading to the lower stories. To remedy this difficulty it is customary to introduce slides into the longer flues by which a part of their capacity may be cut off. A curious misapplication of the above practice recently came to the writer's notice. School-house No. 11 is a new building, occupied about January 1st. It was found that no hot air could be brought into the rooms of the first floor, and that currents of air actually passed from those rooms into the flues. An examination into the matter revealed the fact that, the flues for the different floors were all of the same size and that the slides for regulating the supply had been placed in those leading to the first floor instead of to the floor above. The result was as just stated, the air passed from the rooms of the first floor into the flues leading to that floor, down to the furnace, and then up to the second floor. The first floor was, therefore, ventilated directly to the second, a condition of affairs neither conducive to comfort on the first floor nor good health on the second.

Obviously the better way is to decide how much air each flue is required to supply, and then apply the formulæ above given for cross-section, &c. In this way a constant balance of forces may be maintained.

It is a well-established principle of mechanics that every force acts along the line of least resistance, and no better illustration of its truth can be found than in the application of the laws of movement of heated, confined air-currents.

In order to farther illustrate the theory of ventilation as here set forth, a numerical application will also be made to the Assembly room in the city building of the City of Rochester.

Preliminary data is as follows :

Length $=108'\ 0''$
Breadth$=\ 78'\ 0''$
Height $=\ 27'\ 0''$

Floor area, with an addition for stage $=8831$ square feet.

Total volume, including stage, without deduction for furniture, &c.$=244{,}949$ cubic feet.

The seats now in place accommodate comfortably 1200 people, with standing room for 400 more. Total capacity is therefore taken at 1600. Deducting one-half cubic foot for each seat, and 3 cubic feet for space actually occupied by each person, and making other deductions for cornices, raised floor at sides and ends, projection of stage into room, etc., and we have an available volume of 230,500 cubic feet.

Under the above conditions the floor area for a single person will be

$$\frac{8831}{1600}=5.52 \text{ square feet;}$$

while the volume for each person will be

$$\frac{230500}{1600}=144 \text{ cubic feet.}$$

Lighting apparatus consists of 112 gas burners, each consuming 4 cubic feet of gas per hour. The consumption of gas per hour, then, is $112\times4=448$ cubic feet.

As previously shown a cubic foot of gas in burning produces 0.43 cubic feet of carbonic acid. Production of carbonic acid per hour from combustion of gas, therefore$=448\times0.43=192.6$ cubic feet. Each person produces 0.6 cubic feet of the same gas per hour. Production from respiration, then$=960$ cubic feet. Total amount of carbonic acid per hour$=960+192.6=1152.6$ cubic feet.

Assume the heat from a single person to be 475 units per hour, and the heat from combustion of a cubic foot of gas to be 750 units: We have

$1600\times475=760000$ heat-units per hour from the audience; and from the lights

$448\times750=336000$ heat-units per hour, making a total production per hour of

1096000 heat-units.

Suppose the air supply to be at a temperature of 62° and equal to 2000 cubic feet per hour for each person, the amount per hour will be

$1600\times2000=3200000$ cubic feet.

The amount of heat required to raise a single cubic foot of air 1° at an original temperature of 60° or thereabout is

$0.0766\times0.238=0.01815$ units.

The increase in temperature of the air supply, due to the heat from lights and people will be found as follows:

$\frac{3200000}{13}=246154$ pounds of air per hour, and

$\frac{1096000}{246154}=4.4=$number of heat-units to each pound of air.

A single heat-unit raises one pound of air through

$\frac{1.0000}{0.238}=4°.2$ actual temperature.

The increase of actual temperature of whole supply will therefore be

$4.2\times4.4=18°.48.$

On the supposition that the adjustment of supply to exhaust is properly made the air on leaving the hall must have a temperature of

$62°+18°.48=80°.48.$

The exit per second will equal

$$\frac{3200000}{3600}=888 \text{ cubic feet.}$$

Assuming the velocity of exhaust in vertical flue to equal 20 feet per second, we have for area of said flue

$$\frac{888}{20}=44.4 \text{ square feet,}$$

or what would be preferable, two flues each with area of 22.2 square feet. Assume farther that the inflowing air shall have a velocity of 4 feet per second, the area of inlets will be

$$\frac{888}{4}=222 \text{ square feet.}$$

Assume a sectional area of 6 square feet for each inlet, and

$\frac{222}{6}=37=$number of inlets.

Assume again that velocity in outlets is 6 feet per second,

$\frac{888}{6}$ = 148 square feet = area of outlets.

Also let area of each outlet be 6 square feet, same as inlets

$$\frac{148}{6} = 25 \text{ number of outlets.}$$

The discussion has taken no account of the increase in volume due to the increase of temperature. Were such account taken the dimensions of outlets would be somewhat increased.

The number of changes per hour under the above conditions may be determined by two methods, either by dividing the supply per hour for each person by the volume for each person, thus,

$$\frac{2000}{144} = 13.9$$

or by dividing the total supply per hour by the total available volume of the hall, thus,

$$\frac{3200000}{230500} = 13.9$$

giving the same result in either case.

The discussion has also proceeded upon the supposition of no loss by conduction and radiation. We will now consider the modifications of the above calculations due to these sources of loss.

The hall is warmed by steam, direct radiation. Number of sets of radiators 18, each having an area of 60 square feet. Total radiating surface, therefore,

$$60 \times 18 = 1080 \text{ square feet.}$$

Amount of space heated by one square foot of surface equals

$$\frac{230500}{1080} = 213.4 \text{ cubic feet,}$$

which is certainly ample for the coldest weather likely to be experienced here.

To find loss by conduction through walls and windows, we will assume inside temperature at a mean between temperature of entering air and temperature to which it is raised by heat from persons and lights, or say at 71°. Outside temperature is taken at 32°. The outside exposures are three in number, two sides and one end. We will consider the loss of heat only from the three outside exposures, neglecting the protected end, ceiling and floor.

Area of the three exposed sides exclusive of windows = 6306 square feet. Area of windows = 1696 square feet.

Taking formula (18) we have for the loss through walls exclusive of windows:

a = 6306 square feet,
t' = 71°,
t = 32°,
k = 0.095,
d = thickness of wall = 2 feet,

then

$$Q = 0.095 \times 6306 \frac{71-32}{2} = 11487 = \text{number of heat-units conducted away through walls per hour.}$$

For windows we have

a = 1696,
t' = 71°,
t = 32°.

Experiments have been made in the case of window glass, showing that neglecting d the value of k varies from 1 to 3. We will take it for this calculation at a mean value, namely, 2. We have therefore

$$Q = 2 \times 1696(71-32) = 132288.$$

hence the total loss by conduction per hour under the conditions of temperature assumed is

$$11487 + 132288 = 143775 \text{ heat-units.}$$

Suppose however, instead of allowing 2000 cubic feet per hour for each person, we take what is more nearly the actual supply, that is 850 cubic feet. With that assumption we have a total supply per hour of 1,360,000 cubic feet. The amount of heat from people and lights will be the same as before. Assume temperature of air supply at 62° as previously. Amount of air in pounds

$$\frac{1360000}{13} = 104615.$$

Number of heat-units per pound of air is

$$\frac{1096000}{104615} = 10.4,$$

making an increase of actual temperature of

$$10.4 \times 4.2 = 43°.68.$$

Assuming no loss by conduction and radiation, and the temperature of the outflowing air is

$62^\circ + 43^\circ.68 = 105^\circ.68$.

In fact loss by conduction, &c., increases in proportion as inside temperature increases, consequently the amount conducted away will be correspondingly greater than in the preceding case. Observation will show air in upper part of room to range from 85° to 95°. The writer has on several occasions observed it higher than 80° in lower part of room, and this too with windows open, and outside temperature at from 35° to 40°.

A partial exhibit has now been made of the main principles underlying the Mechanics of Ventilation. In conclusion it will be well to collate principles and facts for convenience of reference.

1. Perfect ventilation is hardly automatic. A certain amount of attention is necessary to keep any system in working order.

2. Ventilation by draught is preferable to ventilation by forcing air in, it having the advantage of supplying fresh air as fast as foul air is removed, provided of course the design is properly carried out.

3. The vacuum may be produced in the case of rooms or suites of apartments by fire-places or by small flues properly connected with the rooms at or near the floor, in which gas jets are kept burning whenever ventilation is needed. In large buildings, however, the vacuum will of necessity be produced by vertical shafts designed in accordance with the principles herein contained.

Corollary.—Where gas jets are used the amount of heat from one cubic foot of gas may be taken as given in the beginning of this paper. Knowing the amount of air to be carried away per hour, the size and number of burners is easily found.

4. Perfect ventilation will cost something. To lift a thousand pounds of air 50 feet, requires exactly the same expenditure of force as to lift a thousand pounds of iron, or any other substance 50 feet.

Corollary 1.—Since to ventilate a building means there is work to be done, and consequently an expenditure of force necessary, any system professing to ventilate without such expenditure of force can hardly be other than a failure.

Corollary 2.—Careful study of the matter shows the importance of reducing friction in flues, ducts, elbows, etc., to a minimum; otherwise a large percentage of force will be expended for that purpose only.

5. Ventilation is a branch of mathematical investigation, actual construction should, therefore, be preceded by careful calculation.

6. Heating is a branch of ventilation and should always be considered in connection with it.

Corollary 1.—In warming by heated air the current should be introduced at or near the ceiling. It is a violation of first principles to introduce it near the floor.

Corollary 2.—In designing a system of warming by heated air, provision should be made for introducing cold air in connection with the warm current, in order that the air may not enter the room at too high a temperature.

Corollary 3.—The cold air currents as per Corollary 2 should be under perfect control as well as the heated currents, in order that the proper temperature inside may be maintained under every variation of outside temperature.

Corollary 4.—Warming by steam or hot water, by a combination of the two, or by steam and hot air combined may all be exceedingly healthful, provided adequate ventilation accompanies them. These methods are all reasonably economical as regards consumption of fuel.

Corollary 5.—Warming by ordinary stoves cannot be considered desirable except on the score of economy. The objections to their use are :

(1) Difficulty of producing ventilation in connection with the warming.

(2) Cast iron, the usual material for their manufacture, when red hot allows the gases, carbonic acid and hydrogen to pass through into the rooms.

(3) The heat is irregularly distributed causing great variations in temperature in different parts of the rooms.

7. It should be understood that ventilation by opening doors and windows is nearly as bad as no ventilation.

8. In large buildings much satisfaction, and what at the present time is of the greatest importance, much valuable information could be obtained by putting up in connection with such a system of ventilation by the vacuum method, apparatus for determining and accurately

registering the results under varying conditions of outside and inside temperature. An anemometer in the main shaft with electric recording apparatus in the superintendent's office, would show at any instant exactly the condition of the ventilation. In this way regularity of action will be assured, and at the same time a collection of data of the greatest value in future building operations can be made.

Farther, in conclusion it may be stated that, so far as the writer is aware, there is nothing in the present paper especially original with himself, except possibly one or two of the combinations of formulæ, and the only reason that can be urged for supposing originality in even that direction is the fact that such combinations have never fallen under his observation. The object of the paper therefore, is to partially systematize our knowledge of this important subject, at the same time putting that knowledge in a form for convenient use. Such systematization is necessarily crude owing to the undeveloped condition of the subject.

The expert will recognize at a glance the great indebtedness to the works of Weisbach and Deschanel.

General Morin's paper before referred to contains valuable information upon this subject, and has been frequently drawn upon in the preparation of this paper.

ON THE EXCLUSION OF SEWER AIR.

By RICHARD WEAVER, C. E., F. C. S., Sanitary Surveyor.

From "Journal of the Society of Arts."

THERE are various reasons why the more perfect exclusion of sewage emanations from the interior of buildings has not attracted that close attention of sanitary engineers which the national importance of the subject demands.

The magnitude and the emoluments of out-door works entirely dwarf the comparatively trifling matter of the internal arrangements of domestic drainage, and the indifference of the public to the subject, combined with the simplicity of faith and unwarranted assumption that things out of sight are right, have certainly not hitherto been very encouraging to the reformer. Those gentlemen who are professionally engaged under sanitary authorities well know the repugnance evinced by tenants and landlords to carry out valuable suggestions, incurring, it may be, trifling expense, often, indeed, in instances where grievous defects are long tolerated and the benefits are mainly for those who prove the most obstructive.

In another direction the same apathy to the advantages of improvements beneficial to health are shown, from a source altogether devoid of official character. A gentleman largely interested in property has had some houses placed in a sound condition in respect of drainage and water supply, not with a view altogether of deriving benefit but more as a test to gauge the susceptibility of his *clientèle*. But he informs me that the great majority evince no interest in the affair, and are more concerned as to the shade of the papers and the tint of the paints, than in the quality of the respirable air and the drinking water. Nevertheless, as the schoolmaster is abroad, and the diffusion of the laws of health, if slow, is established on a sure basis, this impassiveness must cease ere long. It is not far back since a want of knowledge of the principles of hygiene operated as a source of retardation of advancement; but the abundance and general excellence of the literature of sanitation, and the frequent reference to such subjects in the columns of the daily press, sufficiently disposes of the plea of ignorance; whilst a gradual awaking of the public sense to the imperfections of current modes of treating domestic offices, especially when that is combined with an acquaintance with the fact that the remedies are not expensive, in ameliorating existing buildings, which in new structures amounts to nothing

additional upon the usual charges, will unite in good season to leaven the present universal indifference. It is a sign of the times, that will be earnestly read by the people, that a most eminent and popular statesman, rises in his official capacity, and from his exalted position declares to the world, that the sanitary condition of this metropolis and of the country is not satisfactory—a fact long known, of course, to those gathered here in conference, and which will not be lost upon the masses to whom the information is conveyed. I would venture humbly to go further, and to say, and say it boldly, that the condition of many of the metropolitan buildings, against which it may be thought presumption to raise a breath of aspersion, are in an eminently unsatisfactory condition, and where the more urgent is the necessity of observance of sanitary laws, the less they are in practice.

An enterprising, plodding, prying, hard-working Charles Dickens might open out a revelation of evil influences within public institutions and private dwellings—existing for the most part in blind faith, which seems to me inexcusable—as would, beyond doubt, shock the public sense of the proprieties.

Let me ask whether it is known to be anyone's business in particular to be intelligently acquainted with the state of the water supply, and the condition of the local drainage, with the means adopted against the entrance of sewage air? Let me go further, and ask whether it is anyone's particular duty—not nominally, but actively—to become responsible for the satisfactory quality of, and to maintain the breathable air and drinkable water of any establishment reasonably clean and wholesome?

The ladies of the duster and knights in livery are held strictly accountable for the satisfactory appearance of the furniture, floors, and stairs, with other needful matters of the household, but in the best ordered mansions that not unimportant item, the domestic water, is left to the careful attention of cockroaches and spiders, buried away in some dark and for the most part barely accessible position, revelling in an atmosphere replete with exhalations from the common sewers, conveyed through the waste pipe of the cistern. The general atmosphere of the house, too, being a cheap commodity, and apparently not worth consideration, is left to chance for governance.

True, it is charged with organic pollution of cloac, an origin freely admitted through the numerous vents in the scullery, baths, water-closet, and slop basins, mingled with the vapours of cooking viands, and flavored with a *soupçon* of burnt gases. But what of that? Such matters are universal; we rather like it; it keeps us warm during the prevalence of these cutting east winds, and why should our children not do likewise?

Unclean and expired air, with damp, are accountable for 100,000 annual deaths in the shape of bronchitis and consumption. Unwholesome air and polluted water dispose of another 150,000 or so of lives carried off by zymotic diseases. But then this may be a useful provision, and tends to allay the too rapid growth of population, and as every man dies sooner or later, some folks think the sooner the better, and so, upon the whole, things are best left alone. A great deal has been said and written upon the potency of fixed and volatile organic substances in the stages of putresence upon the mortality of the country, much of which I agree with. I know of no one who is more keenly sensitive than myself to the inconvenience of breathing tainted air or drinking foul water, but I cannot approve of the often-uttered expressions by men, even of experience and influence, such as the "deadly sewer-gases," and "death in the cistern." Their opinions, given with good intentions, defeat their object, for people are apt to think that the shades are drawn deeper than requisite, and if the dangers were so great as presented, there would be fewer of us left to talk about it, considering that we all take our peck of dirt in the form of aërial and solid sewage rather frequently. It has been my privilege for some years past to describe the sanitary conditions of many provincial towns in the columns of a journal long interested in such matters, and I think that our country friends will not be averse to a description of the shortcomings of metropolitan household arrangements, for grievous although the former may be, they are at least equaled in the latter.

A careful examination of the substantial buildings in London in any direction

reveals glaring evils, probably causing much injury in the aggregate, although the average death rate would seem to indicate that the general damage sustained is not great. From this knowledge, gleaned during many years' investigation, and an acquaintance with the fact that we are, whilst within doors, always breathing a sewage-tainted atmosphere —and this building is no exception—I am convinced that the actual injury from infringement of the laws of health is much less than is often represented to be the case.

There are congregations of populations within my knowledge who regularly consume their own filth, and drink up the fluid refuse with the water. Places where the receiving cesspits are in such near juxtaposition with the domestic well that the water lines become identical, and intermittent exchange is maintained, and yet many of these places are considered healthy, and figure so in the Registrar's returns. It is only when the virus of specific forms of disease is introduced that death descends, and, like the cock-sparrow on the ant-hill, gobbles up the people right and left and round about him.

An example is within recollection of a small town where nothing more loathsome can well be conceived than the social habits of the inhabitants, where, for 14 years, the community enjoyed good health before an imported epidemic attacked more than a third of the population, of whom many died. There are numerous instances where families and communities exist under the most unfavourable circumstances; but such is the repellent power of the human system, that evils are borne with year by year, apparently without injurious results, and this is probably a reason why so many people are indifferent to and actually resist improvements. Englishmen are fond of games of chance, and there is a charm in taking the odds against the risk. Ten men may live to an advanced age, they may breathe sewer air and drink sewer water every day of their lives, and in the end not more than two of them die of diseases which, as we now reckon such things, can be attributed to disregard of the laws of hygiene. The danger is nevertheless, considerable, more especially in young children, and I think it may be within the experience of most of us that whole families have been swept off by causes quite preventable. In again addressing the public upon this subject, I would refrain from calling attention other than in passing to the dangers to health and the irritation to temper, incurred by living under insanitary conditions, and I would rather take up other ground, and appeal to the high sense of the proprieties characteristic of English women and men to the high value attached to the exercise of scrupulous cleanliness; and in doing so, if I wound the sensitivenesss of some and raise any feelings of disgust, it must be borne with me that I simply state the truth of facts —I do not create them.

The remedy for prevailing errors lies in a recognition of their existence, less by legislative action than by reform within the bosom of every family without distinction, and it should rightly begin with those who sit in high places, for my range of observation, if limited, is sufficiently wide to enable me to say, with some confidence, that the larger and more important residences are relatively in a worse condition than the humbler abodes of the working classes, causing necessity for a more frequent change of air and locality, due to the foul state of the atmosphere of metropolitan mansions, a condition of things rarely suspected, but universally existing. If it may be said, there is not much detriment to health from the present state of dwelling houses, if viewed from the point presented by reference to the mortality rates, I still venture to think that much of that indefined indisposition of families—headache, nausea, dyspepsia, lassitude, and such small complaints, are often created by breathing the foul atmosphere of the house, a foulness, most likely, not perceived by the usual inmates. At all events such effects are produced upon myself and family, and I observe that children become rosy-cheeked and firm fleshed when they are supplied with good fresh air whilst in the house or school—not draughty air—and especially in the dormitories. Possibly, the time even may come in the future, when the sanitary man will be as freely consulted upon the remedy of prevention of such matters as our medical *confréres* now are for their cure.

There is no occasion for me appearing rude, and I have no intention of being so, in the following criticisms, but I am induced to submit them, so as to bring home to every one the fact that, notwithstanding we pride ourselves on our decency and cleanliness, and our inclination to set ourselves up as patterns to Continental neighbors, I say, the fact is, we are a dirty and an unclean people. Indeed, could we but see ourselves as we really are, I think we might even admit we are a filthy folk, and deserve to be scourged. There is a handy phrase that pointedly excepts the present company always, but I will take no shelter under that conventionalism, and I say that the charge is against me and you, and I believe every family in the kingdom. Some medicines are disguised with saccharine matter of less or more cloying sweetness, but I have no sugar to offer, and you must take my facts as I find them.

I repeat this in letters large writ, and hung up that those that run may see and read, for the proceedings of this Conference are not confined within these four narrow walls. If a hound strays from our heels and laps up the soil from the street, left by neglected children, he is switched and called a brute; and if a servant were guilty of polluting the domestic beverages with the contents of the slop-pail, the act would be considered a heinous sacrilege. But practically we do this every day, and what do we care when the foulness of our water-closets is injected from the containers or the soil pipes to the cisterns, and mingles with the water we consume, which I am able to say, from personal examination of many hundreds of houses, actually occurs in nine out of ten residences not of recent erection. We boast of our civilization, and send forth missionaries abroad to teach the heathen. In the good old book of Leviticus are excellent sanitary morals, but do our ministers cull from them their full excellence? It is often alleged by speakers, and I have heard it said in this hall, that the errors of old buildings do not exist to nearly the same extent in new erections, and especially greater improvements are effected in the storage of water. It would be interesting to know how this conclusion is arrived at, whether as an effort of observation, or conviction, or mere hearsay. As regards the latter, I know of no direction in which fallacy is more persistent than in respect to house sanitation.

There is a fixed determination amongst house-holders that things must be right, and that they are right. But a little reflection in the mind of every responsible head of a family will show there is no special dispensation, and careful search would show that gross defects exist.

A few days ago, I made an inspection of a medical gentleman's house—a man who is a frequent attendant upon social conferences, and well read in matters of hygeia. Well, when I came to the water cistern, he assured me the waste pipe delivered into the ground a little below the surface, but as I make it a point to take nothing for granted in my examinations, and must have personal demonstration, we opened out the subsoil and found the overflow orifice discharged into a drain directly communicating with the sewer up which air of a fetid character arose. In point of fact, the waste pipe acted as the upcast shaft to ventilate the house drains and public sewer, the delivery taking place in the cistern an inch or two above the water line.

Of course, seeing the defect himself—one of many—the fact was confirmative, otherwise I think it would have been difficult to convince this gentleman of the nuisance. In new residences there is an average improvement upon the water service storage, for I find that seven houses in ten only are contaminated locally by connection with the sewerage, or with facilities for the absorption of gaseous sewage at some point.

If a guest enters a drawing-room with boots spotted with honest mud, he is, peradventure, looked upon askance; but it is a matter of no moment that the host immediately charges his lungs with abominations vomited forth from the common sewers, through defective closets and scullery sinks, with which the atmosphere of the house is tainted, curiously hidden in vapor of preparing viands ascending from the kitchen, which no mansion, however modern, seems without. We are, forsooth, a peculiar people, and our notions of decency seem sadly erratic.

It is a singular thing, and, perhaps, suspected by few minds, that the con-

ditions essential for the enjoyment of sound, robust health, least exist in localities where they are most required; and I put it to you as the fruit of observation, that our hospitals, as types of public institutions, are the most indecent in respect of their sanitary measures; whilst the gin palaces, as representative of another class, are the best. In the first, I include public buildings, houses, churches, clubs, schools, hotels, and coffee-houses, with many others; and, in the second, the business parts of butchers', bakers', greengrocers', confectioners', with some other shops. Basing the calculation of averages from an examination of several hundred buildings, made within the last year, I am in a position to say that 99 per cent. of metropolitan dwellings are polluted very seriously by the admittance of sewage air through the various openings for the removal of liquid refuse. In many are no trapping arrangements whatever for keeping back aërial sewage, which flows uninterruptedly into the house, often, indeed, to the extent of tens of thousands of cubic feet per day, and the odd thing is that this is always unsuspected by those who should know it, and it is not thought of much consequence when pointed out. But let me ask whether this is common decency to charge the respiral air of a mansion with the exhalations of filth, and to breathe the same, whilst professing some sentiments of cleanliness and refinement? If there were any adequate means of air ventilation of houses, the nuisance now enveloped in the vapors of cooking would not be so great; but there is no systematic attempt and the internal atmosphere, from basement to garret—and the higher up the more stagnant—is generally fetid with foul air, more especially during the nocturnal hours. So general is this defect, that, on admitting a guest, he is met at the front entrance of the establishment with a gush of mixed air, tainted with smells from the kitchen and drainage evolutions, which a trained nostril has no difficulty of identifying. The proper ventilation of houses—that is, the introduction or pure, fresh air without draughts, and the speedy removal of consumed air, so as to maintain the inner atmosphere in a condition approximating to the outer air—has occupied my attention during many years, and as the objects sought for are accomplished, I hope soon to find an opportunity of making known the system, which the scope of this Conference does not now permit.

Some recent discoveries relative to the state of certain modern—I was going to say model—Government offices, the Mansion-house, and others, have been looked upon as alarming, if I understand the matter, but I can say with some knowledge that, if the published accounts represent all the defects found, then the efficiency of those buildings is certainly above the average; and as there seems no palpable reason for their being so, I am constrained to think it is just possible that other indecencies might have been brought to light, if the search had been more closely followed.

Specimens of new dwellings were described in the *Journal* of this Society a few months ago, and since then I have seen others equally defective and indecent, and the samples now furnished refer to older buildings, of jottings from my note-book on the spots, and may best serve to illustrate the average condition.

In a mansion at Regent's-park, which the owner considered perfect in its sanitary appointments, and with that object had expended considerable sums, deleterious gases from the drains entered to the extent of several thousand cubic feet per day. The service cistern contained a filter covering the whole of the bottom, so that all water withdrawn for supply underwent the formula of cleansing—even that taken for water-closet use; but, with the logic of irony, the waste pipe discharged directly into the public drains, the trumpet pipe communicated openly with the pan of water-closet, whilst each action of the valves squirted a jet of water from the closet container into the cistern of potable water.

In a West-end club, noted for the scientific attainments of its members, I found the interior of the premises in direct contact with the street sewer, from which some portion of the air supply was derived with which the visitors are regaled. At all events, through a single untrapped aperture, mephitic vapors flowed at the computed rate of 12,000 cubic feet per day. Most of the water cisterns are polluted in various ways, the greater part of the waste pipes going

right into the drains, whilst one cistern supplying the cook with his requirements, is flavored with an injection of water shot up from the pipe serving the water-closet, each time that necessary convenience is used by the servants.

Into a well-known restaurant near Regent-street there is as much sewage air entering as would seem sufficient to ventilate the sewerage system of a small town, seeing that in the cellarage alone it is estimated that the intake of foul gases equals 30,000 cubic feet; and the exhalations from the closets, urinals, sinks, and lavatories add to the sum total. On inquiry from the superintendent of a department, the atmosphere of which appeared highly charged with impurities, what was the tone of health of the staff, and whether any of them died? He said none of them stayed long enough for that. Just by way of taking off the rawness of the gaseous sewage the establishment arrangements are very complete for distributing the kitchen fumes and the burnt air from probably thousands of gas jets throughout the building, and for this liberal supply of lung food I believe the management most considerately make no charge upon the guest.

In a block of substantial houses, which were in perfect sanitary efficiency—so the architect and surveyor assured the owner for whom he acted—I found in each of them sewer air of a peculiarly fetid character, entering at the rate of several thouand cubic feet *per diem*, distributing its noxious qualities amongst the domestic water and air.

A very fair example of the condition of a London hospital is found in one upon which I lately reported, and the medical staff should be better judges than me whether the arrangements are conducive to the health and convalescence of patients. The drainage system ramifies below the basement floor the length of the hospital, rising by vertical pipes to the wards and upper premises, and between the sewer at one end and the wards and chambers at the other, there is, through the elongation of pipes, no trap intervention whatever, so that perfect channel communication is established between them. The fact is, as no air ventilation is provided, the deficient supply is augmented from the sewer, from which the soil, bath, and lavatory conduits act as up-cast shafts, thus ventilating the sewers into the hospital wards.

Some of the cisterns are also polluted by exhalations, one that supplies the dispensary, I understand, rather considerably so from the closet, as the motion of the lever discharges foul air in bubbles through the water stored in the cistern.

The saturated air of the hospital, stagnant from want of motion, with no apparatus for extraction, without inlets for fresh air beyond the usual crevices, is particularly unpleasant to sensitive lungs. Many examples might be furnished of similar examinations, but the faults are alike, differing only in detail, and the effects are not dissimilar, viz.: —indelicate adulteration of air and water, the cheapest but most necessary elements of existence.

Before passing on to the practical object of this paper, the means available for effectually avoiding the current nuisances common to all dwellings, I will intrude upon you one more illustration of the indecencies of modern living, taken from the residence of a sagacious, well-informed gentleman of eminence—who, always interested in social subjects, is not unknown in this hall—which I was invited to inspect so recently that the evils pointed out still remain unameliorated. In the first place, in the kitchen, as there is no open fire-place, but close stoves and hot plates, the whole of the volatile culinary products are discharged into the basement, the bulk of which ascend to the upper premises. There are no ventilating appliances in this palatial establishment, but a fair amount of air of a sort is procured from outside the building, conducted through the yard grids, and thence by the scullery-pipes to the several passages of the house. Another supply, voluminous in quantity, is emitted from the soil-pipes of the water-closets, another source being through the waste-pipes of the service cisterns. The air of the whole building —of the magnificent chambers of great length and height, and of the upper offices, is singularly stale and unpleasant. Nevertheless, the servants express perfect satisfaction, and profess to enjoy excellent health; but, if this is so, are the

conditions of living proper—are they decent even?

It is many years since I became acquainted with the inadequacy and insecurity of the usual hydraulic traps applied to house drainage to stop the back flow of sewer air. At the most, and under the best conditions, assuming perfect joints, and with sound materials, they merely obstructed the rush of air; the passage from the drains to the house took place more slowly and insidiously. Well, after laboring for some time for improvement, I adopted a simple device formed out of a siphon glazed ware pipe, with an opening at the socket, and communicating with the ground surface for the entrance of fresh air, and then, by the aid of openings at the tops of stack, bath, and soil-pipes, to keep up a system of natural and self-acting air circulation throughout the drainage; so that any passage effected through the porous pipes, joints, or traps—very little, perhaps, by reason of the equal tension of the air within and without the pipes—would be robbed of virulence, because the air, being ordinary atmospheric air, and not sewer emanations, is quite harmless. The idea was taken from—and, in fact, was an attempt to adopt underground—the system not much practised in London, but common in provincial towns, of severing connection with the scullery, by delivering the pipe into the air over a trapped grating, but which had never been carried into practice, so far as I know, with water-closets, nor with drains passing through the house.

As the arrangement has become known through the professional press, and by the proceedings of a kindred association, and possibly is known to you, I need not further describe it. I may, however, say that with some years' experience of its working it gives me every satisfaction, as it thoroughly effects complete severance between the house and sewer, preventing the admittance of any foul air and pollution to water, the whole being accomplished at a cost which, I think, is not immoderate considering the substantial benefits derived; and that it does not generally exceed £5 per house in occupied buildings, in new structures practically amounting to nothing extra upon the usual drainage outlay.

Some question has arisen as to the feasibility of implied action of the siphon system of ventilating and trapping drains and sewers, which is best answered by stating the result of many hundred examinations. The air circulation is produced by compound causes—first, we have the currents due to the natural mobility of the air, as exemplified in all vertical shafts open at each extremity, and well illustrated by a chimney flue without a fire. This motion is accelerated by the passage of water down the pipes, creating a reversed aërial current, and again by the warm discharges from the kitchen. The upward flow is augmented by the heat absorbed from the sun's rays by the ventilating pipes. But the most potent agent in keeping up the circulation is the wind blowing squarely across the mouth of ventilating pipes, creating an exhaust and consequent upcurrent, for it is old knowledge that the rapid passage of a fluid across the orifice of a tube reduces the tension within that tube.

Under exceptional circumstances and local obstructions, there is occasionally a down draught through the pipes, which act as the long leg of the siphon system of ventilation, but this is of no moment, for the outlet being lower down and outside the building, the aerial discharge takes place there after sweeping through the drain pipes, and is generally devoid of smell, for I find that, after a few weeks, operation with fairly laid glazed pipes, although they may have been down for years, the oxidising effect of the continuous body of fresh air passing over the surface deposits within the pipes, entirely consumes the putrid matters. The currents however, are generally ascendant, and deliver at the roof level. A series of observations conducted under varying conditions and localities determine the average velocity of flow at three to four lineal feet per second in calm weather, which increases to nine feet, and often 12 feet with a strong wind. And it is to this severance of the house from the sewer by the water seal of the siphon trap, in conjunction with the sweeping air currents maintained through every drainage pipe of the house, that I depend for clean air and clean water within the building, and is most certain in its action; whilst the means I have devised for the effective

disposal of the locally generated exhalations—the expelled breath of the body, the burnt gases, the emanation from the walls, paper, furniture, and miscellaneous effects stored upon the premises—admitted on all sides as an object well worthy of attention—as I have said, I will take another opportunity of describing.

ON FROZEN DYNAMITE.

By Capt. PHILIP HESS.

From Selected Abstracts, translated for the Institution of Civil Engineers.

Nobel's blasting oil belongs to the numerous class of artificial products, extensively used for industrial purposes, of which the physical and chemical qualities are scarcely understood. But the small number of experiments which have been instituted to bridge over the gaps in the knowledge of so dangerous an explosive can hardly be a matter of surprise.

The difficulty of such a study chiefly arises from the fact that it is rarely possible to time the stages in the combustion of the explosive.

The use of Nobel's material, and especially that of nitro-glycerin when frozen, has greatly added to the above difficulties. The freezing point of nitro-glycerin varies between 39° Fahr. and 53.6° Fahr., and this arises from the differences in manufacture and the varying nature and action of the oil under cold. Respecting the first cause, the Author's investigations show that the oil usually sold never contains 18.5 per cent. of nitrogen, the quantity necessary for tri-nitro-glycerin; therefore in the oil at least two kinds of nitric ether must be assumed to be present; but, on the other hand, there may exist all the three ethers theoretically possible, viz., mono-, bi- and tri-nitro-glycerin; this fact will explain some peculiarities which would otherwise seem paradoxical. The Author begins by tabulating briefly the observations made in respect of this matter.

The blasting oil, when free, will bear sometimes for days low temperatures without solidifying. Samples from Nobel's factory in Zamky remained liquid in the Military Laboratory under temperatures varying between 32° and 17.6° Fahr.

Blasting-oil samples would not, it was found, solidify when exposed even for hours to cold varying from 10° to 5° Fahr., according to Champion. Dr. Gladstone has noticed that nitro-glycerin, when subjected to cold produced by strong carbonic acid and alcohol, will only become thick or glutinous without freezing.

From experiments in the Author's laboratory, it was found that in a considerable quantity of nitro-glycerin, exposed for several days to a temperature between 50° and 32° Fahr., some crystals had detached themselves, remaining for a time floating in the oil, which did not itself congeal.

Contact with absorbent substances, such as a silicious marl, undoubtedly hastens the freezing of the nitro-glycerin in temperatures below 53° Fahr.

Nitro-glycerin, when fluid, renders the silicious admixture pellucid, but when frozen it loses this power; so that thoroughly congealed nitro-glycerin appears no longer flesh-colored or brown, but white. This peculiarity indicates whether a dynamite cartridge is frozen completely. For this it must be broken, which, according to Mowbray, can be done without danger.

If a cartridge is exposed for a day in a parchment box the oil will completely freeze in a temperature below 10° Fahr., if the cartridge has not a greater diameter than 1 inch.

If dynamite is spread over metal plates 1 millimèter thick, half an hour's exposure produces solidification. When blasting oil and nitro-glycerin powder are frozen, and then subjected to shocks which would explode them in a softer state, peculiar variations ensue. The general result of the trials showed that nitro-glycerin and blasting preparations formed from it become less sensitive to mechanical shocks directly the nitro-glyc-

erin contained in the preparation is completely frozen. During the experiments made by the Military Committee, the preparations in question (silicious dynamite being chiefly dealt with when the temperature of the surroundings did not exceed 10° Fahr.) were cooled by artificial means (mixtures of snow and common salt being used), the temperatures obtained being between 1.4° and 69° Fahr., and at which the samples were kept and completely frozen.

Under these conditions the silicious dynamite showed the least liability to explode when tried with a capsule filled with fulminate of mercury, employed to bring about combustion with safety; for the similar reason a cartridge made of gun-cotton and nitro-glycerin was effectively made use of.

With dynamites composed of absorbent matters having a combustible tendency, it was noticed that the diminution in liability to explode was rather lessened when frozen; for example, this was the case with Nobel's dynamites Nos. 2 and 3, where the absorbent material is composed of wood and saltpeter, or with the gun-cotton and dynamite before alluded to. Cartridges of this last-mentioned material exploded, when not frozen and frozen, with the same effect, from the initial shock of the mercury capsule before referred to, when this last contained 9.25 troy grains of chlorate of potash.

Silicious dynamite, when fired at, and soft dynamite, when freely exposed, would not explode when struck with a bullet from the old form of rifle at a distance exceeding 2,500 paces; at 2,000, combustion was produced. Frozen dynamite, however, would not explode even at sixty paces. The Author gives details of the trials by striking or ramming dynamite samples, which were carried out by the Military Committee.

The various preparations were in layers of one millimèter high each, and placed on a plate of 1.34 square centimèter, where they were struck perpendicularly above and below by a steel hammer, the force of the blow being regulated by the amount necessary to explode the sample. Silicious dynamite exploded, when soft, with a blow of 0.75 kilogrammèter; but when frozen this force was increased to one kilogrammèter, and one blow sufficed. Under similar conditions gun-cotton dynamite, when not frozen and frozen, required blows of 0.5 and 1.25 kilogrammèter respectively. With nitro-glycerin powders in a transition state of freezing, the force of the stroke producing combustion is not sensibly increased until complete congelation sets in. With samples in a transition state, explosion is produced at a second or third blow, when the force is less than would be required to effect combustion at a single stroke, and these light blows may be repeated until the strength of the preparation is so much reduced that no result after a time ensues. The limit between explosion and non-explosion is considerable; but when in a soft state the tendency to combustion is more defined.

The results of the Committee's trials are shown in the following table; the weight is eleven lbs., the dynamite being not frozen:

Fall,	19.7	inches	Explosion.
Fall,	15.7	"	"
Fall,	11.8	"	"
Fall,	7.9	"	"
Fall,	5.9	"	"
Fall,	5.9	"	"
Fall,	5.9	"	"
Fall,	3.9	"	No explosion.
Fall,	3.9	"	2 blows... "
Fall,	3.9	"	3 blows... "

Experiments with dynamite in a transition state, between liquid and frozen:

Fall,	19.7	inches	Explosion.
Fall,	15.7	"	"
Fall,	11.8	"	"
Fall,	7.9	"	"
Fall,	5.9	"	"
Fall,	3.9	"	No explosion.
Fall,	3.9	"	2 blows... Explosion.
Fall,	2.0	"	No explosion.
Fall,	2.0	"	2 blows... Explosion.
Fall,	1.6	"	No explosion.
Fall,	1.6	"	2 blows... "
Fall,	1.6	"	3 blows... "

The sensibility of frozen nitro-glycerin to mechanical shocks is shown by these experiments to be less than that of non-frozen nitro-glycerin. In the transition state, however, this sensibility is still further increased, especially in respect to repeated light shocks, such as those accruing through the operation of packing for transport; the experience both of the laboratory and the factory confirms this view.

In dynamite factories, most accidents

take place upon the filling of the cartridges with the nitro-glycerin powder, and when the temperature of the atmosphere fluctuates. To obviate this difficulty, the sheds where the cartridges are made are now heated with water, as the agent best fitted to distribute the temperature equably, the unequal heating caused by ordinary fires being a source of danger.

Mowbray first proved, on a large scale, that dynamite can be carried about, shaken, or broken, with impunity when in a frozen state; and experiments have been instituted to show whether powdered nitro-glycerin in a free state behaves differently from the liquid at low temperatures. Experience proves that with oil in a powdered form congelation takes place more easily than when free, owing, perhaps, to its greater cohesion. It must, however, be remembered that with oil combined with absorbent materials the latter are bad conductors of heat, and therefore a greater degree of cold is required to solidify them. Chemical analysis also proves that nitro-glycerins contains different ratios of nitrogen, and for that reason they have different freezing capacities. It should be remembered, too, that glycerin will only solidify under—40° Fahr., and that Ott lays down the highest point at which rigidity is possible at 53° Fahr.; it should, therefore, be allowed that the freezing point of any three possible nitrates lies between 53° and 40° Fahr. As a rule, in a mixture of different fluids, the one least sensitive to cold prevents the others from freezing, so that the zero point of tri-nitro-glycerins is reduced, according to the quantitative relations of the three nitrates. Several facts tend to elucidate the manner in which nitro-glycerin crystals are formed, which partially influence the cohesive strength of nitro-glycerins when in a powdery form.

Frozen dynamite, as a rule, secretes a portion of its oil when thawed; a portion collects in the lower part of the silicious marl and renders it extremely fatty, sometimes causing a deposit. When thoroughly thawed, re-absorption of this oil sometimes takes place. The above series of experiments show that dynamite, in a transition state, more easily emits nitro-glycerin when under pressure than it does in a soft condition.

If silicious dynamite be placed under water, the oil is separated, owing to the greater affinity of the water to the marl, and at the moment of separation it is more easily frozen, a fact which was proved in the laboratory.

When all these facts are combined, it seems to show a series of harmonising observations, such as the comparative absence of danger in handling large masses of frozen dynamite, its insensibility to the shocks from a gun or a hammer, and to the mechanical caloric impulses of the initial explosion. The result to be deduced seems to be, that with nitro-glycerin in general, and therefore, probably, in each of its three gradations, it is more indifferent to shocks, both mechanical and caloric or chemical, when in a frozen than a fluid state; although it remains to be proved whether it is so under all circumstances, in particular whether well-formed nitro-glycerin crystals are able to resist destructive impulses equally well in all directions of cleavage.

With the oils of commerce, as a rule, congelation takes place partially, and under the influence of lengthened cold the process of thawing is slow and gradual; in practice, it is difficult to determine whether nitro-glycerin powder is wholly frozen until it has been observed and exposed for a considerable time. Such portions of the oil as are frozen seem to alter the relations of cohesion of those portions still fluid with their absorbent material, so that this absorbent seems only feebly able to retain the fluid portion at the now reduced surface. These parts being no longer protected as before by their surrounding absorbent material, are merely loosely embedded between inelastic frozen hard particles, and evidently are more exposed to mechanical impulses than when the whole of the material is in a state of complete solution. The less the absorbent quality of the mixing powder, and the less it has taken up, the more its cohesion has been reduced by the presence of moisture, and, therefore, the more easily the oil will be secreted, and the greater consequent danger during manipulation; this may, it is true, be much reduced by a superabundance of the absorbent, and by well-dried powder mingled with it; but the danger will exist during the greater part

of the cold season, owing to the single cohering particles having but a weak heat-conducting power. The Author insists much on the necessity for equally heating all laboratories where nitro-glycerin is handled. It is certain, from these investigations, that frozen dynamite is less liable to explode than when it is fluid, and that it is more sensitive still in a semi-condition, especially in respect of continuous mechanical impulses. The natural reticence of manufacturers has prevented authentic statistics of accidents and their causes being formed, and consequently a rational code of restrictions on the manufacture and transport of this dangerous material has not yet been devised; but the Author strongly urges the importance of the appointment of good technical chemists to superintend and inspect manufactories, &c., and considers the appointment of untrained government officials highly injurious to the public interest.

MOMENTUM AND VIS VIVA.

By J. J. SKINNER, C.E., Ph.D., Instructor of Mathematics in Sheffield Scientific School, New Haven, Conn.

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IV.

V. THE MEASUREMENT OF CONTINUOUS FORCES.

MUCH confusion has arisen from the use of the word *force* in mechanics in several different senses. It is sometimes possible, even in science, to use a single word in various senses, making the context of any treatise show what meaning the word must have; but the use of the word *force* has been so thoroughly muddled that, even in reading authors usually clear, one is often at a loss as to the precise meaning, *if any*, which the writer has in mind. Sometimes *force* is defined as any agent or cause which produces, or in any way changes, the motion of a body; sometimes it is taken as something which can be measured in pounds, or their equivalents; sometimes as something to be measured in foot-pounds; sometimes something measured by the acceleration produced in masses, as by gravitation; and we have lately had the eminent Prof. Tait, in his lecture to the British Association, (see *Nature*, Sept., 21, 1876), define *force*, not as something to be *measured* by the rate of its doing work, but as being *itself* simply the *rate of doing work*. Prof. Tait attempts to justify this definition of the word by reference to Newton's *laws of motion*; but I cannot but think that the Professor, as suggested by W.O.P., (*Nature*, Oct., 26, 1876), has confused the idea of *measuring* something with that of *being* something. For he admits in the same lecture that the only allowable meaning of the word *force* in those laws is "any *pull, push, pressure, tension, attraction or repulsion.*" This must, by the Professor's previous statements, be capable of measurement in pounds; for he says that "the British *unit of force* is about the former weight of a penny letter—half an ounce." Can this weight be regarded only as a *rate of doing work?*

In the previous articles of this series I have used the word *force* sparingly, and where used without qualification I meant to leave no doubt that it was used as equivalent to *pressure* or *tension*,* measurable in *pounds*. I shall continue to use it with this meaning; and I hope to show that this is the meaning, and the only meaning, that the word must have in Newton's second law.

The fundamental standards which it is necessary to establish in a system of mechanics are the unit of time, the unit of space, the unit of mass of matter, and the unit of pressure or tension, called the unit of force. These *units* must remain invariable; or if any new unit for measuring time, space, matter or force is introduced, it must have a known and invariable ratio to the original unit. Any three of the four units may be taken at

* Except perhaps in the phrase "correlation of forces," where I should possibly have done better to have said "conservation of energy."

our own pleasure, but nature imposes a connection between the fourth and the others.

The unit of time has been thoroughly agreed upon by all civilized nations, viz: the mean second. The unit of space is chosen by each nation to suit itself; in England the common unit of space is the foot. There are two distinct ways of deciding on the two remaining units of mechanics, either of which might be adopted. First, we may choose at pleasure a unit of mass, and then make the unit of force depend on this; or, second, we may choose at pleasure a unit of force, and then determine our unit of mass to correspond. By the former method, English writers conveniently take for the unit of mass, in mechanics, the quantity of matter in a standard piece of platinum, called the pound, which is carefully kept by the Government in a certain place, as a basis of reference in the comparison of quantities of commodities to be exchanged in commerce. Adopting this standard of mass, there is still some choice as to the most convenient way of making the *unit* of *force* depend upon the unit of mass. One way is to assume that the unit of force shall be the amount of pressure, due to gravity, exerted downwards by this unit of mass, when at rest in the specified place of keeping. We say *in this place*, because if the same piece of platinum were carried to a different latitude it would exert by gravity a different downward pressure. The pressure produced at the place of keeping may be taken on any spring balance, and the stress or tension shown by the index may be called a *pound* pressure; and whatever variations may occur in the weights of bodies at different localities, the *pound pressure* in any system of mechanics ought to be as invariable a unit as the standard piece of platinum. The context of any treatise must always show whether pressures or masses are meant by the term pound.

Retaining, as above, the standard pound of platinum as the unit of mass, late writers have introduced another way of determining the unit of force, and have sought to dignify it by calling it an *absolute* unit of force. They say, let us connect the unit of force with our other units by the following principle: The unit of force shall be *that force which by acting for one unit of time on one unit of mass shall give it one unit of velocity.* There is no objection to this method in itself, and there ought to be no difficulty in finding out accurately the relation between the unit of force thus determined, and the pound pressure before defined. The real trouble that the rule makes, is that many students are liable to get from it the idea that this new unit is a different *kind* of force from the pound, whereas the only difference is as to *amount*. The *kind* of force measured is identical, just as much as a gallon of water is of the same kind as a barrel of water; and in fact, this new unit of force is about the same fraction of a pound that a gallon is of a barrel. One English pound of pressure is equal to 32.1912* of the new units, and either the pound pressure or this new unit of pressure will produce motion, or be held in equilibrium, according to the circumstances of any special case.

The remaining method, which has long been taught, of fixing upon the units of mass and force for a system of mechanics, consists in choosing a standard unit of pressure, and then deriving the unit of mass from it. This is the method which I took at the beginning of this series of articles, though without claiming that it has much, if any, advantage over the previous methods. By this method we assume the unit of force to be the pressure due to gravity, produced at the appointed depository, by the quantity of matter known as the commercial standard pound. This pressure is to be observed on a spring balance or other dynamometer, and the position of the index marked as unity. Instead of afterwards retaining as the unit of mass the quantity of matter in the commercial pound, it is thought more convenient for some purposes of calculation in mechanics to take a different quantity of matter as the unit, viz: *that quantity of matter which, when free to move, and when acted on for one second by a pressure of one pound shall acquire a velocity of one foot per second.* From this definition it follows, as explained on page 129† of the current vol.

* This is the value of g at London. Atkinson's Ganot's Phys., p. 13.

† If account is made of the variations of g, that explanation requires the pound weight to be determined always by the spring balance.

of this magazine, that g pounds of matter, weighed anywhere by a standard spring balance, is the unit of mass. This will be an unvarying unit of mass; since, although at various localities g will vary, it will vary in such a way that the quantity of matter which by its weight will pull or drive the balance to a pound tension, will be inversely as g.

Some writers, (Thomson and Tait, Nat. Phil., Vol. 1, p. 166) say that in the common system followed in modern mathematical treatises on dynamics, the definition of the unit of mass is such as to make it variable at various places. But if any writer on dynamics has given a definition that can be so understood, he has simply made a slip; for it would be too great an absurdity to *measure* dynamical quantities by a fluctuating standard. The formula $M=\frac{G}{g}$, by which in this method we determine *mass*, will give the same value for the same body at any place on the earth, if the value of G obtained by a standard spring balance, and the value of g for that place, be substituted. The quantity of matter in the *unit* of mass, determined as now explained, will be equal to 32.1912 times that in the standard pound of platinum; for this would be the quantity to which, if free to move, the standard pound pressure would in one second impart a velocity of one foot per second.

It would seem desirable to have a short and convenient name for this unit of mass. Why can we not call it a *matt?* The *matt* will then be a quantity of matter equal to 32.1912 times that of the standard pound of platinum.

I see no good reason for calling one particular unit of force the *absolute* unit. For if it be said that the pound pressure, even as now defined, would vary with possible variations of gravity at London, we might require it to be determined once for all at a particular time; and its results in maintaining the distortion of a spring, and in producing motion could of course be recorded. Furthermore, the so-called absolute unit has to be determined practically by observations on pendulums, falling bodies, &c.; and any considerable alterations in the gravitation force of the earth would alter the results of these, and undoubtedly also the actual length of the unit of time, on which, together with the arbitrary units of space and mass, the so-called *absolute* unit of force depends.

As to practical convenience there may sometimes be advantage in using this unit of force. If I wished to use it at New Haven, I should need a standard spring balance, graduated by comparison at London with the pressure produced there by the standard pound of platinum; or, if I had here a copy of that standard piece of platinum, I could graduate my own spring balance to give the "absolute" unit, by suspending the standard by the balance, and noting the position of the index. By dividing the distance from the zero point to this position by the value of g found here by experiment, I could get the distance which would corrrespond on the balance to the "absolute" unit of force.

But whichever of the previous modes of defining the *units* of force and mass is taken, these units must be so defined, in any one system, as to be invariable, and we must understand that the unit of force in one system is a certain definite number of times greater or less than in the other, and they may both *measure* the same kind of force, whether it produces visible motion or not. If we are to use the "absolute" unit, ought we not to give it a shorter and better name, and have its ratio to the pound pressure, as previously defined, definitely understood? Would there be any objection to calling this unit a *tend?* This word might be regarded as suggestive of *tendency* or *tension*. If the word were adopted, and a pound pressure were defined as the pressure due to gravity, exerted now at London by the standard pound of mass, experiments prove that the *tend* would be equal to a pound pressure divided by 32.1912; or, one *pound* pressure equals 32.1912 *tends*.

We cannot do better than to quote a few words from the preface to Newton's Principia, to show the object he had in view in formulating the *laws of motion*.

"All the difficulty of philosophy seems to consist in this—from the phenomena of motions to investigate the forces of nature, and then from these forces to demonstrate the other phenomena; and to this end the general propositions in the first and second book are directed. In the third book we give

an example of this in the explication of the System of the World; for by propositions mathematically demonstrated in the first book, we there derive from the celestial phenomena the forces of gravity with which bodies tend to the sun and the several planets. Then from these forces, by other propositions which are also mathematical, we deduce the motions of the planets, the comets, the moon and the sea."

It seems to me that writers on mechanics of late years have made difficulty in this subject by taking what ought to be regarded simply as a means of solving a problem, and turning it into a fundamental definition.

What we want is simply a rule by which from given changes of the motions of bodies we may infer the intensities, as measurable in pounds, of the acting forces, or by which if the intensities of acting forces are given in pounds, we may infer the resulting changes of motion. But instead of stating, as Newton's second law, properly understood, does state, that *changes* of momentum, in bodies free to move, are *proportional* to the intensities of acting forces (pressures or tensions), the law is generally turned about nowadays, and given in the following form:

"*The measure of a constant force is the product of the mass moved by the velocity imparted in a unit of time.*"

This would not be so objectionable, except for the fact that it gives the idea that we must measure forces *only* by momentum produced, and thus that there is an essential difference between the nature of forces which produce motion and of those which appear simply as pressure. Thus it comes that there are plenty of writers on mechanics who will gravely state that forces are of two *kinds*, those which produce motion and those which produce pressure; just as if the imparting of motion to a body were done by something whose intensity could not be measured in pounds pressure, and as if it were not an acting pressure which really produces motion*. We are apt not to see that in *measuring* or comparing forces by this rule the comparison has reference solely to the precise attribute, quality or quantity, which we compare when we measure forces by observing the points at which they will hold a spring balance compressed or extended; and that the rule furnishes only *one way* of measuring forces, to be used for estimating pressures or tensions actually operating to cause motion, in cases where it is inconvenient or impossible to measure those pressures or tensions by a spring balance or other dynamometer. But Newton, after giving his eight *definitions*, adds a scholium, near the end of which, in discussing true and apparent motions, the following occurs:

"If two globes, kept at a given distance one from the other by means of a cord that connects them, were revolved about their common center of gravity, we might from the *tension of the cord* discover the endeavor of the globes to recede from the axis of their motion, and from *thence* we might compute the quantity of their circular motions. And then if any equal forces should be impressed at once on the alternate faces of the globes, to augment or diminish their circular motions, from the increase or decrease of the *tension of the cord* we might infer the increment or decrement of their motions." And further on he says, "But if we observed the cord, and found that its tension was that very tension which the motions of the globes required, we might conclude the motion to be in the globes, and the bodies [among which they were apparently moving] to be at rest; and then, lastly, from the translation of the globes among the bodies, we should find the determination of their motions. But how we are to collect the true motions from their causes, effects, and apparent differences; and, *vice versa*, how from the motions, either true or apparent, we may come to the knowledge of their causes and effects, shall be explained more at large in the following tract. For to this end it was that I composed it." Newton immediately goes on from this to state his famous *axioms*, or *laws of motion*. Near the beginning of his *System of the World*, he again shows the object of his *laws* in nearly the same words as before quoted: "And how from the motions given we may infer the forces, or from the forces given we may determine the motions, is

* Whether all pressure may not be due to innumerable molecular impacts I do not undertake to say. But even if it is, it is something which the engineer has to measure and allow for in pounds, and the word *force* in the rule is simply one name of it.

shown in the two first books of our *Principles of Philosophy*."

The *law of motion* as stated by Newton, or the rule above given for measuring force, ought to be regarded then merely as a means for solving certain problems, and must not be understood as being a definition of the only proper way to *measure* what we call simply *force*.

To learn conclusively that the word *force*, as used in that rule, means simply pressure or tension, may be done by experiments with Atwood's machine. You have simply to balance two equal masses on the cord, and then place a small additional mass on one of them. So long as you prevent motion of the bodies, the small mass will press on the one beneath it with a pressure equal to its weight, and the motion of the system which will follow on releasing the apparatus, will depend on the pressure, equal to the weight in pounds or ounces exerted by gravity on the small unbalanced mass. You arrange a catch by trial, so that after moving for one second the small extra mass shall be caught away, and then observe how far the balanced bodies move in the next second. You then experiment again by placing the small mass once more upon one of the balanced bodies, and removing from the other one-half as much also, which is transferred to the first, so as to leave unchanged the total mass which is to move, but making the difference between the two sides twice what it was in the first experiment.* The effective pressure of gravity producing motion in the system will now be twice what it was before, and it will be found that this pressure, acting for one second, will give to the moving mass twice the velocity before acquired. And in general, if the total mass remain unchanged the velocity imparted in a second will be found to be proportional to the difference between the two suspended masses, and therefore proportional to the effective pressure exerted in moving the system.

To show that the constant pressures necessary to give in a certain time a stated velocity to different masses must be proportional to those masses needs no experimental proof; for we may readily conceive of equal masses placed side by side and acted on by equal parallel pressures, which will produce in the masses equal velocities; and the sum of the pressures will be proportional to the sum of the masses, whatever be their number or the total mass.

Since then a constant pressure, acting for a unit of time, must be proportional to the velocity imparted by it to a given mass, and also to the mass to which it imparts in a unit of time a given velocity; if we change both the mass to be acted on, and the velocity to be imparted in a given time, the pressure must be *proportional* to the product of the mass and the velocity imparted. By taking suitable *units* of mass, velocity and pressure, the *numerical value* of the pressure in pounds may be rendered the same as that of the product of mass by velocity imparted in the unit of time, and thus this product may be taken, in certain cases, as a convenient way of comparing or *measuring* pressures, as stated in the rule. If a constant pressure acts for a *unit* of time on the *unit* of mass, (g pounds of matter) free to move, the velocity imparted will be equal in *numerical value* to the number of pounds in the acting pressure, and is called the *acceleration*, and sometimes the *accelerative effect* of the force.

This last is apt to be a misleading name. For the word *effect* is more frequently used, especially by practical engineers, to apply to the *work* done by a force; they are therefore liable to be confused, and to fail to distinguish clearly between *work* and that which we compare by *acceleration*. Now the *acceleration* of the unit of mass, due to any force, will depend simply on the intensity of this as measurable in units of pressure, whether the body acted on be originally at rest or moving with any velocity; but the *work done* in producing any *acceleration* will depend on the original velocity. If that is zero, it has been shown in the first article of this series that the work will vary as the square of the velocity afterward imparted.

The failure to recognize that in measuring forces by *accelerations* we are concerned with nothing but *intensities*, as comparable, at least allowably, in pounds, leads even highly educated physicists

* We neglect the mass of the moving wheels of the machine, since their motion will be uniformly accelerated by the same laws as that of the balanced masses; and friction is made so small as to be almost insensible.

into what seems to me needless perplexity. The following paragraph is from a paper by Prof. Henry Morton, Ph. D., in the *Journal of the Franklin Institute* for November, 1868.

"We say and know that the *vis viva*, or work done by a moving body, varies with the square of its velocity, while we know, by our previous reasoning, that the force expended in giving it that velocity only varies with the velocity itself. Thus the force of gravity will give a falling body a double velocity in a double time, during which it must have exerted a double force upon it. Here, then, we have a double force, doing a quadruple work. Is this because by some wonderful and recondite property inherent in velocity the double power has been indued with an again doubled efficiency? Many writers leave us to think so; but we, on the contrary, believe that the work done *only seems to increase* more rapidly than the power implied in the increased velocity, by reason of a *loss of efficiency* in the resistances, in the overcoming of which the "work" consists, and in fact, that work in this sense, is no true measure of force."

By the phrase "force expended" in the first sentence, the writer could not have meant "work done," and yet he must have meant to speak of a *quantity* of some sort. What the unit of that quantity is, is not quite clear. If he had said, "We know that the vis viva, or work which will be done by a moving body, in coming to rest, varies with the square of its initial velocity, and we also know that if two constant pressures or tensions, acting for the same time, give different velocities to equal masses, the pressures are proportional to the velocities," he would have said what can be proved by experiment; and the statements would not conflict at all with each other, or with the fact that in a *double time* a *constant* pressure, like that of gravity, acting on a body free to move from rest, will perform a *quadruple* work; though certainly not by reason of "exerting a double force," but a constant one. For gravity is of such a nature that it exerts a constant* force (pressure or tension) on a body, whether that is moving or not, and it is able to perform a quadruple work on a falling body in a double time, simply because it exerts this pressure on the falling body through four times the distance in two seconds that it does in one. To be sure, if gravity acted for one second, and then ceased, a falling body would move thirty-two feet in the next second, and the actual pressure in this second is not responsible for this thirty-two feet motion; but *because* of this motion, the constant pressure can make its proper addition to the velocity only by being exerted through this thirty-two feet, besides the sixteen feet which it alone in this second would move the body from rest. Thus the total distance moved by a constant pressure in imparting a double velocity must be quadruple, and the work done in imparting velocity not only *seems* to increase as the square of the velocity, but actually does so increase.

Prof. Morton claims that the *effect* of a *force* must be proportional to the *time* of its action. This would be true of the *effect* of an engine, for example, working at a *constant rate;* but why should it be true of a force like gravity which acts with an intensity of the same number of pounds at all known velocities? Suppose a heavy body to be raised against the action of gravity, by some other force, at a *uniform* velocity. Then suppose another body of the same weight to be raised at a velocity, also uniform, but ten times as great as the velocity of the first. This body will be raised ten times as high in a given time as the first. Would the real *work*, or *effect*, performed against gravity be no more in this case than in the first? If not, it would be a good thing for the engineer to know; for by gearing up his hoisting machinery to the highest possible velocity he could produce the most wonderful results with the least possible force.

There is, indeed, one sort of effect, or *result*, of the action of a constant force, which is proportional to time, viz: the change of *velocity* of a moving body, when acted on by the force. That is to say, if a force of constant intensity act so as to impart motion to a body, the *change* of velocity of the body will be the same in each unit of time, no matter what the velocity actually becomes; so that the total change of velocity produced by a constant force in any number

* Neglecting the slight change due to difference of distance from a center.

of seconds will be proportional to that number of seconds. But simple *change of velocity* is not what is usually understood by the *effect* of a force. Writers have generally agreed to measure the *effect* of a force by *work*, or the product of the *force* and the *distance* through which it is exerted, whether the work is done in imparting velocity to bodies or in overcoming any sort of resistance. It seems to me that we need in mechanics a name which shall apply to *the exertions of force during time*, whether this produces apparent motion of masses or not, just as much as we need the name *work* to denote the exertion of force through distance, independent of velocity. The word *toil* appears to me well adapted to supply the need, as I am not aware that it has hitherto been used in any technical sense in mechanics. I would suggest for the consideration of writers on this subject, that we take the word *toil*, and define it for mechanical purposes, as *the exertion of force* (*pressure or tension*) *during time*. The exertion of a pound pressure for one second, whether producing changes of velocity, or overcoming any resistances, or simply maintaining the distortion of elastic bodies, like springs, etc., might be taken as the unit of *toil*, and called a *second-pound*. If any one preferred to use the *tend* as the unit of force, the unit of toil could be made the *second-tend*.

In another number I will return to a consideration of the use of this word *toil* with the above meaning. At present, as illustrative of the conflicting views and statements to which ill-defined or ill-used words may lead, let us compare an explanation of Prof. Morton's with one from Cooke's *Chemical Physics*. Prof. Morton, *loc. cit.* p. 347, says: "If then, a moving body with a certain velocity, overcomes a certain number of these resistances, or, for example, penetrates a medium to a certain depth, before its motion is arrested, it has overcome so many resistances, each acting for such a length of time. If now the same body with a *double* velocity, meets the same medium, it will penetrate each resisting element in *half the time*, and so receive from it but *half the resistance* it experienced before."

Cooke's *Chemical Physics*, p. 52, says concerning the resistance met by a rail-road train: "With a double velocity the moving train passes over double the space each second, and therefore encounters twice as many points of resistance. Moreover, it strikes each of these points with *double* the velocity, and hence meets at each point *twice the resistance*. It therefore meets, during a second, twice as many points of resistance, and suffers *at each point* twice as much resistance. The resistance during a second is thus four times as great as before and must require four times as much force [whatever that means] to overcome it."

The two views quoted cannot both be right, and it seems to me that they are both wrong, and the truth between them. The mere friction of a rail-road train, not including the resistance of the air, would, within some limits of speed, be almost invariable in intensity. That is, if a train were drawn *at a constant rate*, by a weight hanging over a pulley, then if an outside power gave a higher velocity to the train, and afterwards ceased to act, the same hanging weight as before would keep the train at the new velocity, so far as mere friction is concerned.* Hence the work done in overcoming friction in a given time would not vary as the square of the velocity, but simply as the velocity; the number of pounds in the resistance being *the same* as at the former speed, instead of either *one half* or *double*. The resistance of the air is another matter, and would have to be considered by itself; but at low velocities it forms a very small part of the total resistance, and I believe that no practical engineer would say that in running a train for a day on a level road at the constant rate of six miles per hour he would burn four times as much coal as he would in running for the same time at three miles per hour. In *starting the train* and *getting up speed* the work due to inertia varies as the square of the velocity produced, but this is entirely aside from friction and the resistance of the air. And if doubling the initial velocity of a moving body causes it to

* This is a result reached by the experiments of Morin and Coulomb; and the valuable experiments of Prof. A. S. Kimball, described in the *American Journal of Science* for May, 1877, show that it would be sensibly true within certain limits, though at high velocities Kimball's experiments, as well as the previous ones of Bochet, show that the coefficient of friction would *diminish*, instead of increasing.

penetrate a certain medium to a quadruple depth, it is proof enough that the average intensity of the resistance at the various points of the medium is unchanged.

We have explained how to show by experiment with Atwood's machine, that the word *pressure* or *tension*, as denoting a quantity measurable in pounds, is perfectly suited to take the place of the word *force* in the ordinary rule for the measurement of *force*, and hence that *force* as used in that rule means *pressure* or *tension;* and since the word cannot be supposed to have several meanings in a single rule, we exclude all meanings conflicting with this. The only objection that could be made to the demonstration from experiment would be to inquire, how do we know that gravity exerts the same *pressure* or pull on a body in motion as on the same at rest? Indeed, Prof. Morton, *loc. cit.* would explain the fact that a double initial velocity causes a body to rise against gravity to a quadruple height, by saying that the body, "traversing each distance in half the *time*, gravity would exert but *half* its former *effect* within the same space." But aside from the alteration in the pressure, due to change of distance from the earth's center, and which is so slight that for almost every purpose it may be wholly disregarded, we have no reason to suppose that any possible velocity would alter the intensity of the pressure or pull exerted on a body by gravity. For we find by experiments with Atwood's machine, that the velocity of a falling body *increases* as much in the third or fourth second, as in the first; which shows that the intensity of the pressure does not diminish when the actual velocity has become greater.

If any one should suppose that the action of gravity in the experiments described is peculiar, in that the small mass used to produce motion forms practically an essential part of the mass moved, and thus that there is any uncertainty as to the action of pressure on the whole, we can devise experiments in which a constant pressure or tension shall be applied to a mass through a coiled spring which shall indicate directly the amount of pressure in pounds or ounces, which is active in imparting velocity.

Suppose first a certain mass of matter to be moving without friction on a horizontal plane, drawn by the action of gravity on another mass suspended by a cord passing over a frictionless pulley and attached to the first. Let us suppose this cord to be perfectly flexible and inextensible, and without weight, (if that were conceivable), so that the two masses shall move with identical but increasing velocity. What would be the tension on the cord? If the velocity of the masses is uniformly accelerated, our theory requires that the effective pressures by which they are urged shall be proportional to the masses. Let the mass of the one moving horizontally be M, and that of the one moving vertically be m, the weight of the latter in pounds be w, and the tension on the cord in pounds be t; the effective pressure with which the mass m will be urged, will be the difference between its weight and the tension on the cord, viz: $w-t$, and the tension on the mass M will be t; so that we shall have the proportion

$$t : w-t = M : m,$$

whence we may find

$$t = \frac{Mw}{M+m}. \qquad (4)$$

In our hypothetical case, this formula would enable us to calculate the tension of the cord connecting the two masses. In nature there is no inextensible cord; and the question occurred to me, what would be the result if the mass M were resting on a frictionless plane, held in place by the hand or otherwise, and were solicited horizontally by the action of gravity on the mass m, suspended over a pulley by a cord attached to the mass M by a coiled spring. It is evident that, so long as motion of the mass M is prevented, the tension on the cord would be equal to w. If M be suddenly released, it will then be drawn at the first instant by the tension w, which is evidently greater than $\frac{Mw}{M+m}$. To begin with, therefore, the mass M will follow m with a greater velocity than it ought to have if the two masses are to move with equal velocities. And similarly m will be more retarded if its motion be resisted by the tension w, than it would

be by $\frac{Mw}{M+m}$. Hence the two masses will not immediately move with equal velocities, but in moving will be drawn nearer together. But, as the distance between them becomes smaller, the spring will yield so as to exert a diminished tension on the masses, and this tension may finally become reduced to $\frac{Mw}{M+m}$. Will it then remain at this value? The formula for the inextensible cord makes the tension for given values of M and m invariably this, no matter what actual velocity is obtained. But while the tension on the supposed spring has been reducing itself to this value, the mass M has had more than its proper share of work done on it, and must be moving with considerably greater velocity than the mass m; hence it will continue to approach this, and so will still further reduce the tension on the spring.

This approach of the bodies and reduction of tension will go on until the now greater proportional effective action of gravity on the mass m shall have raised its velocity to equality with that of M. But at this point, the spring will be exerting less than the normal tension due to a common velocity, hence the mass M will now immediately begin to lag behind, relatively to m, and thus the spring will be again elongated. If it were perfectly elastic it would be stretched to the original tension given by the weight of the mass m when at rest, and would then begin another similar oscillation, and so keep on oscillating, as long as the mass M continued to be drawn by the action of gravity on the mass m. The extent of each oscillation must evidently be such as to give a *mean* tension on the spring equal to $\frac{Mw}{M+m}$.

It is difficult to find a plane sufficiently free from friction to test this result, but a similar series of oscillations ought to occur if a mass were suspended by a spring from one of the balanced masses of an Atwood machine, and then these set free to move. In the absence of Prof. Loomis, Prof. Wright kindly placed the fine Atwood machine of Yale College at my disposal, and in the first rough experiments which I made with it, to verify this conclusion, there was no difficulty in detecting the oscillations. I had a coiled spring made, of about 130 turns of fine brass wire, making the length of the spring, when hanging by its own weight, about seven inches; and when supporting a weight of 31.1 grms. its length was about thirty inches. The weight of the spring was 7.43 grms. I attached one end of a silk thread to the top of the spring, and allowed the thread to hang down through the interior of the spring. To the bottom of the thread I fastened a small paper scale of equal parts, so that it would hang by the side of the 31.1 grm. weight. On attaching the spring supporting this weight, to one of the balanced masses of the machine, and releasing the apparatus, this paper scale would fall with a velocity equal to that of the balanced masses, and the oscillations of the 31.1 grm. weight along the scale were easily though not very accurately observed. With a balanced mass equal in weight to 602 grms. the oscillation of the end of the spring was at least $2\frac{3}{8}$ inches. I give no numerical comparison of this result with the formula, as the spring in this experiment formed a part of the unbalanced weight, and the mode of observation was not close. But with a lighter propelling weight and shorter spring, so as to get quicker oscillations, there was no difficulty in observing four or five oscillations before the fall had to be stopped.

I afterwards altered my arrangements, by running a stiff wire down through the center of the spring, attaching the spring at top to this wire, and this in turn to one of the balanced masses, and using a piece of brass tubing for the propelling weight. This hung at the bottom of the spring, and the stiff wire passed through it without touching it. On the wire below the position of the weight when at rest, I arranged a new and finer scale of equal parts. To determine with considerable accuracy the amount of oscillation, I attached a fine silk thread to the bottom of the tubular weight, and allowed it to pass down the face of the paper scale, being lightly held against it by a turn or two of another silk thread around the scale at its top and bottom. A small knot in the vertical thread served to mark its position. This position being noted by

drawing the thread just straight when the weight was hanging at rest, on releasing the apparatus the first oscillation would draw the thread up along the scale, and leave it at its highest position. To reduce friction here as much as possible, when making the observations, I first found by trial approximately the value of the first oscillation, and then by raising the tubular weight by hand, drew the thread nearly to the right height, so that afterwards when raised by the oscillation of the apparatus the work should be almost inappreciable.

I arranged the masses on the machine so as to have all my apparatus except the tubular weight form a part of the balanced mass, the value of this being 330 grms., neglecting the silk suspension cord and the wheels of the machine. The tubular piece (with fine wire hooks for attachment), which produced the tension, weighed 17.63 grms. My friend, Mr. H. A. Hazen, kindly assisted me in releasing one of the masses suddenly from a brass plate below.

After one or two trials to get the approximate oscillation, for the purpose explained, I made five observations of the first oscillation, with the following results:

$1\frac{4}{32}$ inches.
$1\frac{5}{32}$ "
$1\frac{4}{32}$ "
$1\frac{6}{32}$ "
$1\frac{4}{32}$ "

The mean of these five trials, gives for the extent of the first oscillation, 1.144 inches.

Before proceeding to inquire whether one-half of this oscillation of the spring corresponds to a reduction of the tension to the value $\frac{Mw}{M+m}$, it is necessary to take into account the effect of the inertia of the machine, which is very considerable. If we balance two known masses on the cord, and then add a small piece to one of them, the velocity of the fall will be retarded by the inertia of the machine, as well as by that of the balanced masses. But, the velocity of the wheels of the machine being uniformly accelerated, if we could deprive the machine of inertia, and leave friction and resistance of air out of account, there would be a certain particular mass of matter, which, added to the balanced mass, would be just equivalent in resistance to that actually opposed by the inertia of the machine.

Hence we may find the value of this equivalent mass as follows: since in any locality masses are proportional to their weights, let X be the weight of the balanced load on the machine, *including* this unknown equivalent, and suppose the machine to have no inertia. Let w be an extra weight applied to one of the masses to cause motion, d the distance through which it causes the mass to move in a second, and s the distance a body falls freely in the first second. From well known principles we shall have the proportion

$$d : s = w : X+w,$$

whence $$X=\frac{w(s-d)}{d} \qquad (5)$$

from which, by observing d and w, we may calculate X; and knowing how much the separate load on the machine is, we find the equivalent to the machine by difference. This difference ought, so far as simple inertia is concerned, to be the same for all variations of loads and moving weights; but, on account of friction and atmospheric resistance, it is better to determine it by using about the same values as in the particular experiment with which comparison is to be made. By doing so we very nearly eliminate the effect of these other resistances.

With so rapid a fall as that given by the masses of my experiments, it was difficult to determine the precise distance fallen in a second, (we could not be sure in a single trial to get the value within an inch or so), but Mr. Hazen's long practice in observing time by the ticks of an astronomical clock enabled us, by estimating fractions of a second, to get results not far out of the way. We removed the coiled spring, and took first a balanced load of 330 grms., and a propelling weight of 18 grms. I manipulated the machine and allowed twenty falls of the weights through distances varying from five to twenty inches. I numbered each experiment, and noted the distance, while Mr. Hazen, not knowing this latter, sat with his back to me and estimated by sound

the time of each fall, to the tenth of a second. On comparing our lists, and calculating from each observation the distance fallen in one second, the mean of the twenty observations gave 8.1 inches as the value of d. Reducing this to feet, taking $s=16.08$, and $w=18$, the above formula gives for the value of X, 410.81 grms.; from which, by subtracting the load of 330 grms., we find the resistance of the machine to be equivalent to that of a mass equal to 80.81 grms. added to the balanced masses.

We are now in condition to calculate from our formula (4), for the mean tension of the coiled spring, what value we ought to expect for t in the experiments described. And since we may substitute weights for masses in that formula, we write it, $t=\frac{Ww}{W+w}$, in which W must be the balanced load on the machine, plus the equivalent of the inertia of that. We have then, $W=330+80.81=410.81$, $w=17.63$, and by substitution we find

$$t=16.905.$$

I found by trial that when my spring was stretched by a weight of sixteen or eighteen grms., an additional weight of five grms. produced an elongation of 3.75 inches; hence one inch motion of the end of the spring represents a change of tension equal to 1.333 grms. But in the experiments with the spring above described, the extent of the oscillation was found to be 1.144 inches. The tension at the middle of the oscillation, that is at the point of mean tension during the fall, would therefore be found by subtracting $\frac{1}{2}\times 1.144\times 1.333$ grms. from the weight of the tubular mass, which was 17.63. Making this subtraction we find the value of t, as given by observation of the oscillations, equal to 16.868 grms., as against 16.905 found by calculation from the weights used and the estimated inertia of the machine. The difference between these results is 0.037 grms., which though a far greater error than would be allowed in work for any important purpose, is perhaps as small an error (less than five per cent. of the loss of tension) as could be expected without taking pains to construct more perfect apparatus for making the observations. The loss of tension on the spring at the mean point of oscillation is, by the above observations,

$$17.63-16.868=0.762 \text{ grms.}$$

I made a second set of experiments having the same tubular piece for the propelling weight, but reducing the balanced load to 130 grms. The oscillations noted were

$$\left.\begin{array}{ll} 1\frac{15}{16} & \text{inches} \\ 2\frac{1}{16} & \text{“} \\ 2 & \text{“} \end{array}\right\} \text{mean}=2 \text{ inches},$$

showing a reduction of tension at the middle of the oscillation equal to 1.333 grms. A special set of sixteen observations for determining the resistance of the machine and air at the new speed gave it equivalent to 93.2 grms., unless I were to reject four rather doubtful observations; which would have made it 83.66, a value nearer that found for the other speed. If I assume $W=130+93.2$, and $w=17.63$ the formula $t=\frac{Ww}{W+w}$ will give $t=16.339$, corresponding to a loss of tension equal to $17.63-16.339=1.291$ grms., against that of 1.333 grms. indicated by the oscillations; the error being 0.042 grms., or a little over three per cent. If I were to take the other equivalent of the inertia, making $W=130+83.66$, the value of t by the formula would be 16.286, and the corresponding loss of tension therefore 1.344; the difference between this and the result of the oscillations being only 0.011 grms., or less than one per cent.

There would seem to be no reason for expecting the tension on the spring to ever return in the oscillation, so as to exceed that at the beginning, when the masses are at rest. Yet I thought it worth while to try once or twice to see if I could discover such an excess, but was unable to do so. If then, we can find as above, by calculation or otherwise, what the mean tension on the spring during the fall should be, and can begin the experiment by supporting the propelling weight in some way, so as to let it start the fall with only this mean tension on the spring, there ought to be no oscillation either way from this mean. It ought then to go on, exerting this tension on the balanced masses with invariable intensity, no matter what velocity should be obtained, (disregarding fric-

tion, atmospheric resistance, and the slight increase of gravity on approaching the earth's center). During my first experiments with the 31.1 grm. propelling weight, which was found to give an oscillation of as much as $2\frac{3}{5}$ inches, I attached a silk thread to this weight, and by catching it on a screw, turned it so as to draw the weight up $1\frac{1}{8}$ inches from its hanging position. I also arranged another silk thread to prevent the motion of the balanced masses until desired. These threads crossed each other, and could be cut at the same moment by a pair of scissors. On my cutting the threads, Prof. Wright watched the falling weight, and stated that if there was any oscillation it was less than $\frac{1}{5}$ inch. The experiment was repeated with similar result.

The experiments herein described tend to show, what, of course, theory first indicates, that a falling body which is made to exert a pull or pressure on other bodies so arranged that they offer no resistance except that due to their inertia, cannot exert a force on them equal to its weight; the reason being that a part of the real pressure of gravity on the body used as a propellor is resisted by reaction of this body against acceleration. But as the experiments tend to confirm the formula, $t=\frac{Mw}{M+m}$, they also indicate that, for any given values of M and m, the masses might acquire any conceivable velocity, without the slightest diminution of the mean tension exerted between them; since the formula is independent of the velocity.

We are then, I think, justified in expecting that with sufficient care we could, by means somewhat similar to those described, apply to a body free to move, a force which should be shown by a spring balance to be constantly a fixed number of pounds or ounces, and which should give the body a velocity varying directly as the time, and should make the total space traversed proportional to the square of the velocity acquired; thus leaving no room to doubt that the *work* performed on the body had not only apparently but actually varied as the square of the velocity imparted, and thus that the real *work* which the body ought to be able to do, in virtue of its velocity, should be proportional to the square of this velocity.

JOINTS IN WOODWORK.

By Mr. HENRY ADAMS, A.I.C.E.

From "The Builder."

This subject is seldom treated in a sufficiently systematic manner in the text-books used by the junior members of the profession, especially as among engineers iron is looked upon as the staple material of construction, the properties and economical uses of which receive in consequence an almost exclusive share of their attention. Although wood is used much less than formerly in permanent structures, it is still of sufficient importance to warrant a careful study of its nature, properties, and uses.

All trees are divided by botanists into three classes,—*exogens*, or outward-growers; *endogens*, or inward-growers; and *acrogens*, or summit-growers, according to the relative position in which the new material for increasing the substance of the tree is added, viz., whether toward the outside, the inside, or the top. Typical trees of each class would be the oak, the palm, and the tree-fern. We have to deal with the exogenous class only, as that furnishes the timber in general use for construction, the term "timber" including all varieties of wood which, when felled and seasoned, are suitable for building purposes.

If the stem of an exogenous tree be cut across, it will be found to exhibit a number of nearly concentric rings, more or less distinct, and, in certain cases, radial lines intersecting them. These rings represent the annual growth of the tree, which takes place just under the bark. Each ring consists of bundles of woody fibre, or vascular tissue, in the

form of long, tapering tubes, interlaced and breaking joint with each other, having a small portion of cellular tissue, at intervals. Towards the outer edge of each ring the woody fibre is harder, more compact, and of a darker color than the remaining portion. The radial lines consist of thin hard vertical plates formed entirely of cellular tissue, known to botanists as "medullary rays" and to carpenters as "silver grain." As the tree advances in age the rings and rays become more irregular, the growth being more vigorous on the sunny side, causing distortion. It must be borne in mind that the strength of wood "along the grain" depends on the tenacity of the walls of the fibres and cells; while the strength "across the grain" depends on the adhesion of the sides of the tubes and cells to each other.

Tredgold proposed a classification of timber according to its mechanical structure. This, as modified by Professor Rankine, is given in the following table :

Class I. Pine-wood (coniferous trees) : Pine, fir, larch, cowrie, (New Zealand pine), yew, cedar, &c.

Class II. Leaf-wood (non-coniferous trees) : Division I., with distinct large medullary rays ; Sub-division I., annual rings distinct : Oak ; Sub-division II., annual rings indistinct : Beech, alder, plane, sycamore, &c.

Division II., no distinct large medullary rays : Sub-division I., annual rings distinct : chestnut, ash, elm, &c. Sub-division II., annual rings indistinct : mahogany, teak, walnut, box, &c.

Having glanced at the microscopical structure of the wood, we shall be in a position to understand the process of seasoning and the shrinking incidental to that operation. While wood is in a growing state there is a constant passage of sap or nutritive fluid, which keeps the whole of the interior of the tree moist and the fibres distended; but more especially towards the outside. When the tree is cut down and exposed to the air the moisture gradually evaporates, causing the fibres to shrink according to certain laws; this is the natural process of seasoning. There are various methods of seasoning timber artificially; in each case the object in view is to expedite the process of evaporation. The shrinkage in length is very slight, and need not therefore be considered, but the shrinkage transversely is so great that it is necessary to look closely into the nature of it, as the question of jointing is affected considerably thereby. The behavior of timber in shrinking was demonstrated by Dr. Anderson in one of the Cantor lectures at the Society of Arts, of which the author of this paper has availed himself to some extent.

As the moisture evaporates the bundles of woody fibres shrink and draw closer together, but this contraction cannot take place radially without crushing or tearing the hard plates forming the medullary rays, which are unaffected in size by the seasoning. These plates are generally sufficiently strong to resist the crushing action, and the contraction is therefore compelled to take place in the opposite direction, *i.e.*, circumferentially, the strain finding relief by splitting the timber in radial lines, allowing the medullary rays in each partially severed portion to approach each other in the same direction as the ribs of a lady's fan when closing. The illustration of a closing fan affords the best example of the principle of shrinking during seasoning, every portion of the wood practically retaining its original distance from the centre. If the tree were sawn down the middle, the cut surfaces, although flat at first, would in time become rounded, the outer portion shrinking more than that nearer the heart, on account of the greater mass of woody fibre it contains and the larger amount of moisture. If cut into quarters each portion would present a similar result.

If we assume the tree to be cut into planks, then, after allowing due time for seasoning, it will be found that the planks have altered their shape. Taking the center plank first, it will be observed that the thickness at the middle remains unaltered, at the edge it is reduced, and both sides are rounded, while the width remains unaltered. The planks on each side of this are rounded on the heart side, hollow on the other, retain their middle thickness, but are reduced in proportion to their distance from the center of the tree; or, in other words, the more nearly the annual rings are parallel to the sides of the planks the greater will be the reduction in width. These remarks apply more especially to oak, beech, and the stronger home firs. In the softer woods the medullary rays

are more yielding, and this slightly modifies the result, but the same principles must be borne in mind if we wish to avoid the evils of shrinking which may occur from negligence in this respect. The peculiar direction which "shakes," or natural fractures, sometimes take is due to the unequal adhesion of the woody fibres, the weakest part yielding first. In a "cup-shake," which is the separation of a portion of two annual rings, the medullary rays are deficient in cohesion. So far we have considered the shrinking only as regards the cross section of various pieces. Turning now to the effect produced when we look at the timber in the other direction: If we take a piece of timber with the end cut off square, as this shrinks the end still remains square, the width alone being affected. If, however, the end be bevelled we shall find that in shrinking it assumes a more acute angle, and this should be remembered in framing roofs, arranging the joists for struts, &c., especially by the carpenters who have to do the actual work of fitting the parts.

We may now leave the question of shrinkage and proceed to a consideration of the more immediate object of the paper. In the following table an attempt has been made to classify timber under the different terms by which it is known, according to its size and other accidental characteristics. This is only a rough approximation, as no definite rule can be laid down, but it may be of some assistance to those who have occasionally to deal with workmen using the terms.

CLASSIFICATTION OF TIMBER ACCORDING TO SIZE
(*approximate.*)

Balk	12	by	12	to	18	by	18			
Whole timber	9	"	9	"	15	"	15			
Half timber	9	"	4½	"	18	"	9			
Scantling	6	"	4	"	12	"	12			
Quartering	2	"	2	"	6	"	6			
Planks	11	to	18	by	3	to	6			
Deals	9	by	2	to	4½					
Battens	4¼	to	7	by	¾	"	3			
Strips and laths	2	"	4½	"	½	"	1½			

Pieces larger than planks are generally called timber, but when sawn all round are called scantling, and when sawn to equal diminsions each way are called die-square. The dimensions (width and thickness) of parts in a framing are sometimes called the scantlings of the pieces. The term "deal" is also used to distinguish wood in the state ready for the joiner from "timber," which is wood prepared for the use of the carpenter. A "log" or "stick" is a rough whole timber unsawn.

The use of wood may be discussed under the two heads of carpentry and joinery. The former consists principally of the use of large timbers, either rough, adzed, or sawn; and the latter of smaller pieces, always sawn, and with the exposed surfaces planed. The carpenters' work is chiefly out-door; it embraces such objects as building timber bridges and gantries, framing roofs and floors, constructing centreing, and other heavy or rough work. The joiners' work is mostly indoors; it includes laying flooring, making and fixing doors, window sashes, frames, linings, partitions, and internal fittings generally. In all cases the proper connection of the parts is an essential element, and in designing or executing joints and fastenings in woodwork, the following principles laid down by Professor Rankine should be adhered to, viz.:

1st. To cut the joints and arrange the fastenings so as to weaken the pieces of timber that they connect as little as possible.

2nd. To place each abutting surface in a joint as nearly as possible perpendicular to the pressure which it has to transmit.

3rd. To proportion the area of each surface to the pressure which it has to bear, so that the timber may be safe against injury under the heaviest loads which occurs in practice, and to form and fit every pair of such surfaces accurately in order to distribute the stress uniformly.

4th. To proportion the fastenings so that they may be of equal strength with the pieces which they connect.

5th. To place the fastenings in each piece of timber so that there may be sufficient resistance to the giving way of the joint by the fastenings shearing or crushing their way through the timber.

To these may be added a sixth principle not less important than the foregoing, viz., to select the simplest forms of joints, and to obtain the smallest possible number of abutments. The reason for this is that the more complicated the joint, or the greater the number of bearing surfaces, the less probability there will be of getting a sound and cheaply-made connection. To insure a fair and equal bearing in a joint which is not quite true, it is usual after the pieces are put together to run a saw-cut between each bearing surface or abutment; the kerf or

width of cut being equal in each case the bearing is then rendered true; this is often done, for instance, with the shoulders of a tenon or the butting ends of a scarf, when careless workmanship has rendered it necessary. When the visible junction of two pieces is required to be as close as possible, and no great strain has to be met at the joint, it is usual to slightly undercut the parts and give clearance on the inside. In pattern-making the fillets which are placed at the internal angle of two meeting surfaces are made obtuse-angled on the back, in order that when bradded into place the sharp edges may lie close. The prints used by pattern-makers for indicating the position of round-cored holes are also undercut by being turned slightly hollow on the bottom. This principle is adopted in nearly all cases where a close joint is a desideratum. Clearance must also be left in joints of framing when a settlement is likely to take place, in order that after the settlement the abutting surfaces may take a fair bearing to resist the strain.

The various strains that can come upon any member of a structure are: *Tension*, stretching or pulling; *compression*, crushing or pushing; *transverse strain*, cross strain or bending; *torsion*, twisting or wrenching; and *shearing*, cutting; but in woodwork, when the latter force acts along the grain, it is generally called *detrusion*, the term shearing being limited to the action across the grain. The first three varieties are the strains which usually come upon ties, struts, and beams respectively. The transverse strain, it must be observed, is resolvable into tension and compression, the former occurring on the convex side of a loaded beam, and the latter on the concave side, the two being separated by the neutral axis or line of no strain. The shearing strain occurs principally in beams, and is greatest at the point of support, the tendency being to cut the timber through at right angles to the grain; but in nearly all cases if the timber is strong enough to resist the transverse strain, it is amply strong for any possible shearing strain which can occur.

Keys and other fastenings are especially subject to shearing strain, and it will be shown in that portion of our subject that there are certain precautions to be adopted to obtain the best results.

The following tables will serve as an introduction to the remaining portion of the paper:

CLASSIFICATION OF JOINTS IN CARPENTRY.

Joints for lengthening ties, struts, and beams:—Lapping, fishing, scarfing, tabling, building up.

Bearing-joints for beams:—Halving, notching, cogging, dovetailing, tusk-tenoning, housing, chase-mortising.

Joints for posts and beams:—Tenon, joggle, bridle, housing.

Joints for struts with ties and posts:—Oblique tenon, bridle, toe-joint.

Miscellaneous:—Butting, mitreing, rebating.

CLASSIFICATION OF FASTENINGS IN CARPENTRY.

Wedges; keys.

Pins:—Wood pins, nails, spikes, trenails, screws, bolts.

Straps; sockets.

And for joinery must be added glue.

We will consider these joints in the order given above. One of the first requirements in the use of timber for engineering purposes is the connection of two or more beams to obtain a greater length. One method of doing this is by lapping, the two pieces being held together by straps, and prevented from sliding by the insertion of keys. A similar joint may be made by using through-bolts instead of straps, and wrought-iron plates instead of oak keys. This makes a neater joint than the former, but they are both unsightly, and whenever adopted the beams should be arranged in three or five pieces in order that the supports at each end may be level, and the beams horizontal. This joint is more suitable for a cross strain than for tension or compression. The common form of fished beam is adapted for compression; if required to resist tensile strain keys should be inserted in the top and bottom joints between the bolts.

Tabling consists in bedding portions of one beam into the other longitudinally. Occasionally the fishing pieces are tabled at the ends into the beams to resist the tendency to slip under strains, but this office is better performed by keys, and in practice tabling is not much used. The distinction between fished beams and scarfed beams is that in the former the original length is not reduced, the pieces being butted against each other, while in the latter the beams themselves are cut

in a special manner, and lapped partly over each other; in both cases additional pieces of wood or iron are attached to strengthen the joint. A form of scarf adapted to short posts has the scarf cut square and parallel to the sides, so that the full sectional area is utilized for resisting the compressive strain. When the post is longer, and liable to a bending strain, the scarf should be inclined, to allow of greater thickness being retained at the shoulder of each piece, the shoulder being kept square. In this joint a considerable strain may be thrown on the bolts from the sliding tendency of the scarf, if the shoulders should happen to be badly fitted, as any slipping would virtually increase the thickness of the timber where the bolts pass through. The width of each shoulder should be not less than one-fourth the total thickness. Joints in posts are mostly required when it is desired to lengthen piles already driven, to support a super-structure in the manner of columns. Another form of scarf for a post put together without bolts has the parts tabled and tongued, and held together by wedges. This is not a satisfactory joint, and is, moreover, expensive because of its requiring extra care in fitting, but it may be a suitable joint in some special cases in which all the sides are required to be flush. In the common form of scarf, in a tie-beam, the ends of the scarf are bird-mouthed, and the joint is tightened up by wedges driven from opposite sides. It is further secured by the wrought-iron plates on the top and bottom, which are attached to the timber by bolts and nuts. In all these joints the friction between the surfaces due to the bolts being tightly secured up plays an important part in the strength of the joint, and as all timber is liable to shrink, it is necessary to examine the bolts occasionally, and to keep them well tightened up. Sometimes the scarf is made vertically instead of horizontally, and when this is done a slight modification is made in the position of the projecting tongue. The only other scarfs to which attention need be called are those in which the compression side is made with a square abutment. These are very strong forms, and at the same time easily made. Many other forms have been designed, and old books on carpentry teem with scarfs of every conceivable pattern, but in this as in many other cases the simplest thing is the best, as the whole value depends upon the accuracy of the workmanship, and this is rendered excessively difficult with a multiplicity of parts or abutments.

In building up beams to obtain increased strength, the most usual method is to lay two together sideways for short spans, as in the lintels over doors and windows, or to cut one down the middle and reverse the halves, inserting a wrought-iron plate between, as in the flitch-girders. The reversal of the halves gives no additional strength, as many workmen suppose, but it enables one to see if the timber is sound throughout at the heart, and it also allows the pieces to season better. A beam uncut may be partially decayed in the center, and hence the advantage of cutting and reversing, even if no flitch-plate is to be inserted, defective pieces being then discarded. When very long and strong beams are required, a simple method is to bolt several together, so as to break joint with each other, taking care that on the tension side the middle of one piece comes in the center of the span with the two nearest joints equidistant. It is not necessary in a built beam to carry the full depth as far as the support. The strain is, of course, greatest in the centre; and, provided there is sufficient depth given at that point, the beam may be reduced towards the ends, allowance being made for the loss of strength at the joints on the tension side. A single piece of timber secured to the underside of a beam at the centre is a simple and effective mode of increasing its strength. The straps are bedded into the sides of the beams: they thus form keys to prevent the pieces from slipping on each other. This weakens the timber much less than cutting out of the top or bottom, as the strength of a beam varies only in direct proportion to the breadth, but as the square of the depth. The addition of a second piece of timber in the middle is a method frequently adopted for strengthening shear-legs and derrick-poles temporarily for lifting heavy weights.

We now come to the consideration of bearing-joints for beams, the term "beam" being taken to include all pieces

which carry or receive a load across the grain. The simplest of these is the halving-joint, where two pieces of cross bracing are halved together, or where the ends of two wall-plates meet each other. When the joint occurs in the length of a beam, it is generally called a scarf. In each of these examples, half the thickness of each piece is cut away so as to make the joint flush top and bottom. Sometimes the outer end of the upper piece is made thicker, forming a bevelled joint, and acting as a dovetail when loaded on top. When one beam crosses another at right angles, and is cut on the lower side to fit upon it, the joint is known as single notching; when both are cut, it is known as double notching. These forms occur in the bridging and ceiling joists of double and double-framed flooring. When a cog or solid projecting portion is left in the lower piece at the middle of the joint, it is known as cogging, cocking, or caulking. Dovetailing is not much used in carpentry or house joinery, owing to the shrinkage of the wood loosening the joint. A wedge is sometimes inserted on the straight side to enable the joint to be tightened up, as the wood shrinks. Tredgold proposed the form known as the "Tredgold notch," but this is never seen in practice. Tusk-tenoning is the method adopted for obtaining a bearing for one beam meeting another at right angles at the same level. This occurs round fire-places, hoistways, and other openings through floors. The advantage of this form is that a good bearing is obtained without weakening the beam to any very great extent, as the principal portion of the material removed is taken from the neutral axis, leaving the remainder disposed somewhat after the form of a flanged girder. When a cross piece of timber has to be framed in between two beams already fixed, a tenon or chase-mortise is one of the methods adopted. If the space is very confined, the same kind of mortise is made in both beams, but in opposite directions. The cross piece is then held obliquely, and slid into place. Occasionally it is necessary to make the chase-mortise vertical, but this is not to be recommended, as the beam is more weakened by so doing. In some cases a square fillet is nailed on to take the weight of the joists, without cutting into the beam. While speaking of floors, the process of firring-up may be mentioned. This consists of laying thin pieces or strips of wood on the top of the joists or any surfaces to bring them up to a level. Firring-pieces are also sometimes nailed underneath the large beams in framed floors, so that the underside may be level with the bottom of the ceiling-joists, to give a bearing for the laths, and at the same time allow sufficient space for the plaster to form a key. Housing consists of letting one piece of wood bodily into another for a short distance, or as it were a tenon the full size of the stuff. The treads and risers of staircases are housed into the strings, and held by wedges. Housing is likewise adopted for fixing rails to posts. The most common joint, however, between posts and beams is the tenon and mortise joint, either wedged or fixed by a pin. The friction of the wedges when tightly driven, aided by the adhesion of the glue or white lead with which they are coated, forms in effect a solid dovetail, and the fibers, being compressed, do not yield further by the shrinking of the wood. When it is desired to tenon a beam into a post without allowing the tenon to show through, or where a mortise has to be made in an existing post fixed against a wall, the dovetail tenon is sometimes adopted, a wedge being driven in on the straight side to draw the tenon home and keep it in place. In joining small pieces the foxtail tenon has the same advantage as the dovetail tenon, of not showing through, but it is more difficult to fix. The outer wedges are made the longest; and in driving the tenon home these come into action, first splitting away the sides and filling up the dovetailed mortise, at the same time compressing the fibers of the tenon. This joint requires no glue, as it cannot draw out. Should it work loose at any time, the only way to tighten it up would be to insert a very thin wedge in one end of the mortise. Short tenons, assisted by strap bolts, are commonly adopted in connecting large timbers. The post is cut to form a shoulder, so that the beam takes a bearing for its full width, the tenon preventing any side movement. When a post rests on a beam or cill-piece, its movement is prevented by a "joggle," or stub-tenon.

But too much reliance should not be placed on this tenon, owing to the impossibility of seeing, after the pieces are fixed, whether it has been properly fitted. It is also particularly liable to decay from moisture settling in the joint. For temporary purposes posts are commonly secured to heads and cills by dog-irons or "dogs;" the pieces in this case simply butt against each other, the object being to avoid cutting the timber, and so depreciating its value, and also for economy of labor. The double tenon is used in framing wide pieces, and the haunched tenon when the edge of the piece on which the tenon is formed is required to be flush with the end of the piece containing the mortise. Tredgold recommended a bridle joint with a circular abutment, but this is not a correct form, as the post is then equivalent to a column with rounded ends, which, it is well known, is unable in that form to bear so great a load before it commences to yield. A strut meeting a tie, as in the case of the foot of a principal rafter in a roof truss, is generally tenoned into the tie by an oblique tenon, and the joint is further strengthened by a toe on the rafter bearing against a shoulder in the tie. Tredgold strongly advised this joint being made with a bridle instead of a tenon, on account of the butting surfaces being fully open to view. A strut meeting a post, or a strut meeting the principal rafter of a roof-truss, is usually connected by a simple toe joint. The shoulder should be cut square with the piece containing it, or it should bisect the angle formed between the two pieces. It is sometimes made square with the strut; but this is incorrect, as there would, in some cases, be a possibility of the piece slipping out. In ledged and braced doors or gates, this joint is used, the pieces being so arranged as to form triangles and so prevent the liability to sag or drop, which is so difficult to guard against in square-framed work without struts or braces. When a structure is triangulated its shape remains constant so long as the fastenings are not torn away, because, with a given length of sides a triangle can assume only one position; but this is not the case with four-sided framing, as the sides, while remaining constant in length, may vary in position. Among the miscellaneous joints in carpentry not previously mentioned, the most common are the butt joint, where the pieces meet each other with square ends or sides, the mitre-joint, where the pieces butt against each other with bevelled ends, bisecting the angle between them as in the case of struts mitred to a corbel-piece supporting the beam of a gantry, and the rabetted or "rebated" joint, which is a kind of narrow halving, either transverse or longitudinal. To these must be added, in joinery, the grooved and tongued joint, the matched and beaded joint, the dowelled joint, the dovetailed joint, and other modifications of these to suit special purposes. "Flooring laid folding" is a method of obtaining close joints without the use of a cramp. It consists of nailing down two boards, and leaving a space between them rather less than the width of, say, five boards. These boards are then put in place, and the two projecting edges are forced down by laying a plank across them, and standing on it. This may generally be detected in old floors, by observing that several heading joints come in one line, instead of breaking joint with each other. It is worthy of notice that the tongue or slip feather, which in good work is formed generally of hard wood, is made up of short pieces, cut diagonally across the grain of the plank, in order that any movement of the joints may not split the tongue, which would inevitably occur if it were cut longitudinally from the plank.

With regard to fastenings, wedges should be split or torn from the log so that the grain may be continuous, or, if sawn out, a straight-grained piece should be selected. Sufficient taper should be put on to give enough compression to the joint, but too much taper would allow the possibility of the wedge working loose. For outside work wedges should be painted over with white-lead before being driven, this not being affected by moisture as glue would be. In scarf-joints the chief use of wedges is to draw the parts together before the bolt-holes are bored. Keys are nearly parallel strips of hard wood or metal; they are usually made with a slight draft to enable them to fit tightly. If the key is cut lengthways of the grain,

a piece with curled or twisted grain should be selected, but if this cannot be done the key should be cut crossways of the log from which it is taken and inserted in the joint with the grain at right angles to the direction of the strain, so that the shearing stress to which the key is subject may act upon it across the fibers. In timber bridges and other large structures cast-iron keys are frequently used, as there is with them an absence of all difficulty from shrinkage. Wood pins should be selected in the same way as wedges from straight-grained hard wood. Square pins are more efficient than round pins, but are not often used on account of the difficulty of forming square holes for their reception. Tenons are frequently secured in mortises by pins, the pins being driven in such a manner as to draw the tenon tightly into the mortise up to its shoulders and afterwards to hold it there. This is done by boring the hole first through the cheeks of the mortise, then inserting the tenon, marking off the position of the hole, removing the tenon, and boring the pin-hole in it rather nearer the shoulders than the mark, so that when the pin is driven it will draw the tenon as above described. The dowelled floor gives another example of the use of pins.

Nails and their uses are too well known to need description; it may, however, be well to call attention to the two kinds of cut and wrought nails, the former being sheared or stamped out of plates, and the latter forged out of rods. The cut nails are cheaper, but are rather brittle; they are useful in many kinds of work, as they may be driven without previously boring holes to receive them, being rather blunt-pointed and having two parallel sides which are placed in the direction of the grain of the wood. The wrought nails do not easily break, and are used where it is desired to clench them on the back to draw and hold the wood together. The following table gives the result of some experiments on the adhesion of nails and screws:

ADHESION OF NAILS (EXPERIMENTS BY MR. BEVAN).

Description of Nails used.	No. to the lb. avoir.	Inches long.	Inches forced into wood.	Lb. pressure to force in dry Christiana deal.	Lb. pressure to extract from dry Christiana deal.
Fine sprigs	4560	0.44	.40	—	22
Fine sprigs	3200	0.53	.44	—	37
Threepenny brads	618	1.25	.50	—	58
Cast-iron nails	380	1.00	.50	—	72
Sixpenny nails	73	2.50	.25	24	—
" "	"	"	.50	76	—
" "	"	"	1.00	235	187†
" "	"	"	"	end grain	= 87‡
" "	"	"	1.50	400*	327
" "	"	"	2.00	610	530
" "	"	"	"	end grain	= 257
Fivepenny nails	139	2.00	1.50	—	320

Summary.

	Across grain.		With grain.
Adhesion of nails in deal	2	to	1
Adhesion of nails in elm	4	to	3

Entrance to extraction is as 6 to 5.

Common screw .2 in. diameter = 3 times the adhesive force of a sixpenny nail.

Spikes are nearly of the same form as nails, but much larger, and are mostly used for heavy timber work. Trenails are hard wood pins used in the same way as nails. In particular work with some woods, such as oak, they are used to prevent the staining of the wood, which would occur if nails were used and any moisture afterwards reached them. Compressed trenails are largely used for fixing railway chairs to sleepers, as they swell on exposure to moisture and then hold more firmly. Screws are used in

* Or 4 blows by a weight of 6.275 lbs. falling freely through 1 foot.

† These nails required a pressure of 327 lbs. to extract them from dry elm; 507 lbs. from dry oak; 667 lbs. from dry beech; and 312 lbs from green sycamore.

‡ These required a pressure of 257 lbs. to extract them from dry elm.

situations where the parts may afterwards require to be disconnected. They are more useful than nails, as they not only connect the parts, but draw them closer together, and are more secure. For joiner's work the screws usually have countersunk heads. Where it is desired to conceal them, they are let well into the wood, and the holes plugged with dowels of the same kind of wood, with the grain in the same direction. For carpenter's work the screws are larger, and have often square heads. These are known as coach-screws. The bolts, nuts, and washers used in carpentry may be of the proportions given in the following table:

Thickness of nut.....	=1	Diam. of bolt.
....ditto head.....	=¾	ditto.
Diameter of head or nut over sides.....	=1¾	ditto
Side of square washer for fir	=3½	ditto
Ditto ditto oak	=2½	ditto
Thickness of washer.....	=¼	ditto

The square nuts used by carpenters are much too thin, unless they are equal in thickness to the diameter of the bolt. The full advantage of that diameter cannot be obtained, the strength of any connection being measured by its weakest part. A large square washer is generally put under the nut to prevent it from sinking into the wood and tearing the fibers while being screwed up, but it is also necessary to put a similar washer under the head to prevent it sinking into the wood. This is, however, often improperly omitted. Straps are bands of wrought iron placed over a joint to strengthen it and tie the parts together. When the strap is carried round one piece, and both ends secured to a piece joining it at right angles, as in a king-post and tie-beam, it is known as a stirrup, and is tightened by means of a cottar and gib keys. When straps connect more than two pieces of timber together, they are made with a branch leading in the direction of each piece, but they are usually not strong enough at the point of junction, and might often be made shorter than they are without impairing their efficiency; sockets are generally of cast-iron, and may be described as hollow boxes formed to receive the ends of timber framing.

The author has confined himself principally to those joints which are used in general construction; many others might have been mentioned, but not being required in ordinary practice they have been omitted, to avoid extending the paper to an undue length. He cannot conclude without acknowledging the assistance he has derived from the works of Tredgold, Rankine, and others, which contain valuable tables and information relating to the subject under consideration. His thanks are also due to the council of the City of London College for the loan of a set of fifty models of joints. Other joints were illustrated by a few larger models, and one of these, a quadruple dovetail, may be referred to as a curiosity of jointing.

FIREPROOF BUILDINGS.

From "The Engineer."

So-called fireproof buildings were, as most of our readers know, first introduced in Lancashire for the construction of cotton mills. The brick walls carried arched floors of brick, supported on cast iron beams resting on the walls and on intermediate rows of columns. The well-known Murdoch is believed to have devised the general outline of such structures, and their rapid introduction which followed was due not merely to the supposed immunity from conflagration, but to the great increase of stiffness of these beam and arched floors as compared with those of timber previously in use, whereby it was thought that a large per centage of the actuating steam power, previously wasted by the yielding nature of the shaft and gear bearings, was saved. For some time no noticeable conflagration took place in any of these "fireproof" mills, which continued to multiply, with little variation from the original type of structure, until at length one of them took fire; and although it could not be said to have been burnt down as

one of the old timbered, floored and roofed buildings might have been, still the structure was as effectually destroyed, the heavy floors being broken down in succession as the cast iron beams and columns, softened by heat, gave way, and the floors fell upon each other, the walls also being split and distorted by the unequal expansion of their materials. Still, fireproof structures—in name, at least—continue to be produced both for mills and for stores; and amongst the many large examples to the latter purpose were the colossal fireproof structures of the Albert Dock warehouses at Liverpool, in which some very notable improvements in the details of the connections of the columns and beams of the floors, and some decided errors in the design of the beams themselves, were noticeable. It is to be regretted that no account suitably illustrated has, so far as we are aware, ever appeared of those ponderous, and in many respects meritorious structures, the designs of which emanated from the office of the late Mr. Jesse Hartley, then engineer to the docks trustees of the port of Liverpool. During the quarter of a century intervening between 1830 and 1855 several great conflagrations occurred in large warehouses, composed almost entirely of iron and brick, and sometimes with more or less timber in the floors, which demonstrated how small was the title of such structures to be called fireproof. The tide of professional and public acquiescence in the fireproof pretensions of brick and iron structures began to turn, and in a few years more there were not wanting those who were presumed to be authorities who boldly affirmed that a fireproof building was an impossibility. Amongst those who gave prominence to this oracular and sweeping condemnation were the late Mr. Braidwood, of the Metropolitan Fire Brigade, and Mr. Bidder, with a good many others of smaller note as authorities. Some unseen circumstances everywhere—except perhaps in Lancashire—tended to prevent much consideration being given by competent engineers and architects to the subject of fireproofing, prominent amongst which has been the fact that fire insurance companies have deemed it their policy to discountenance any attempt to construct genuine fireproof buildings, as not only sapping the very foundations of their business, but on the grounds that an occasional conflagration is advantageous to them as inducing the extension of fire insurances. Now, we venture to affirm that it has not been proved, and so far is not provable, that fireproof buildings in some form are an impossibility; and moreover, the subject of their construction has never yet met with a truly scientific and adequate investigation. Little progress is likely ever to be made while the general design of buildings proposed to be rendered fireproof continues to follow those of buildings constructed upon long-established models, into which combustible material largely entered, every detail being dictated in fact by the use of such materials, and without reference to the question of indestructibility by fire; nor are we ever likely to make much progress while the minds of architects and engineers are led away from the true problem by paltering with those designs which profess to make fireproof structures out of materials in great part destructible by fire, such as the cheap mongrel constructions in which the floors of large buildings are formed of wood imbedded in concrete, the whole supported on rolled iron joists. Various expedients have been proposed, and to a certain extent adopted, with a view to retard the progress of destruction by fire in buildings into which timber and like combustible materials enter largely as constituents. Such were the inventions of Lord Stanhope, at the close of the last century, those of Fox and Barret, and of a host of others of more recent date, including the patent of Evans and Swain, described in *The Engineer* of 29th ult.; but all these, however partially useful, do not properly belong to the class of truly fireproof buildings, to which our present considerations are limited. These, like the far simpler and cheaper scheme proposed in the last century, of covering the lower side of wooden floors with sheet iron, may no doubt delay the progress of fire, but it is a mere delusion to suppose it can ever form parts of a fireproof structure. The choice of materials available for the construction of a genuinely fireproof building is but small; cast iron, wrought iron, and steel are the only metals available to us; these may indeed constitute the

sole material of even the largest fireproof structures, as in the case of the Custom houses of Valparaiso, designed by Mr. Lloyd, which are of great magnitude and wholly of wrought iron, and which have considerable pretensions also to rank as earthquake-proof—buildings suitable to that shaky country. But in general, large structures must consist in great part of non-metallic materials, that is, bricks or stone. To the former of these we must mainly look, as by its means we may adapt the forms of the integrant parts of the brick-work, so as best to permit, without dislocation, the free expansion and contraction of large masses, and yet prevent, as one effect of rapid heating or cooling, the total dislocation of the structure. The composition of brick-earths, which, in fact, may include all forms of pottery ware, may admit of being so varied as to produce but a minimum change of volume under considerable differences of temperature. Almost the whole of our commonly employed and useful building stones are excluded by reason of their natural rigidity of structure, which causes them when somewhat rapidly heated to spall off, crack, and fly to pieces. Yet much remains to be investigated in respect of this branch of the subject. There are some stones well adapted for building, admitting of division in good flat beds and joints, readily allowing the use of the punch and chisel or stonecutter's saw, which yet may be rapidly heated or cooled without disintegration. Amongst these are certain trappean rocks and those of consolidated ash. There are some metamorphic rocks, chiefly of Silurian age, into which magnesia enters largely as a constituent, which are very infusible, have but a small coefficient of dilatation, and bear rapid heating and cooling, even through considerable ranges of temperature, without becoming split or shattered. Stones of this class, which pass by insensible degrees into steatite, which is found in vast masses in Northern India, is employed in many parts of Europe, Asia, and the United States as a fireproof material for the lining of stoves or fireplaces for domestic or other purposes, and stone of this class are well adapted for the walls of fireproof buildings. Those well acquainted with the products of volcanic countries cannot but be impressed, by the phenomena there often presented, with the fact that many volcanic tufas are capable of being formed into bricks by suitable choice and admixture, so that they may be subjected to certain and violent heating without any cracking or dislocation in a far higher degree than any fire or other bricks hitherto employed; indeed, not far from Bonn, on the Rhine, bricks are made of pumice mixed with other volcanic material, which are largely employed in Germany for oven and furnace building, and are well suited for fireproof structures. The range of temperature through which building structures may be raised by accidental conflagration is not accurately known. Like every other branch of the whole subject, our knowledge is misty, and still awaits a careful investigation by exact methods of the physicist experimenting in concert with the scientific engineer or architect. We may roughly affirm, as experience shows, that the temperature to which burnt-out buildings have been exposed is generally greater as the area of conflagration is greater, and that even in the largest fires the highest temperature reached but just touches the fusing point of cast iron. Some instances have been recorded in which materials have been fused, indicating a temperature considerably in excess of this, but a more careful examination than appears to have been given would most probably show that in these cases the high temperature has been due to local and exceptional causes, such as the storage of nitre or other oxygen-yielding compounds in contact with combustible drugs, such as velonia, resins and the like, or have been due to the conflagration having been urged at certain points by rushing currents of air, produced either by high wind or by upward currents of already heated air. We have thus some data for determining the amount of expansion to which the brickwork or masonry is likely to be exposed. Were we only in possession of a large range of exact determinations of the coefficients of dilatation of the materials themselves, such experimental researches would be the real alphabet of the whole subject of fire-proofing. We then come to the more complex questions as to how we are to combine the integrant parts of the walls and floors so that we shall

admit freely their irresistible expansion and consequent contraction without dislocation or other destruction of the masses of brick or stone work. It would far exceed the scope or space here at command further than thus to hint at the subject to be systematically examined. We boldly affirm, however, that the point has never yet been approached in a truly scientific, and pursued in an exhaustive manner, and that until this shall have been done by really competent men, who, for the work, must possess much other qualifications than those of mere engineers or architects, it is both premature and unwise to dogmatize, or oracularly to affirm, that a fireproof building is an impossibility. We are able to build furnaces many of which are equal in capacity to houses or stores. The limits of form and structure of these have been in the course of long experience determined, so that none need doubt the capability of a new furnace to withstand the effects of the high temperature to which it would be exposed. Why should the principles and methods which have led to success here be impossible when the interior cavity of the structure is designed for something else than merely a receptacle in which fuel is to be burnt? Whether we design a furnace or a fireproof building—which to be really fireproof is but a furnace applied to a new purpose, and perhaps upon a larger scale—two entirely different though intimately connected classes of conditions have to be provided for: no essential part of the structure must soften or melt at the highest temperature to which it would be exposed; the materials of the structure, and the structure viewed as a whole, must be so put together that its parts—chiefly walls, floors, and staircases—must be perfectly free to expand and contract again, while at the same time capable of resisting such extraneous mechanical forces as must be applied to them by reason of the necessary uses of the building itself. We are not here writing a treatise, however brief, upon the practice of fireproof construction—how these conditions, admittedly complex and difficult, are best to be fulfilled; such a treatise to be of the least use, must involve much space, and thoughtful consideration in detail. All we here propose is to point out what are the conditions that must be fulfilled, and to add, chiefly by way of example, a few negative propositions, indicating what must not be done if we are to attain the object. The dimensions of the separate chambers into which the building, however large as a whole, may be divided, must not exceed a certain and very moderate limit; and this must bear a certain proportion to the area and transverse resistance of the floors, and to the construction and mass, the nature of the perforations for the admission of light, etc., of the walls. Some guiding analogy as to all this we already possess in the dimensions which experience has assigned to the thickness and modes of construction of the walls of furnaces, as compared with the size of the heated cavity within. External walls—perhaps, indeed, dividing walls also—should be double, with a space between, so that each wall shall be free to alter its dimensions irrespective of the other. No portion of the floors must be rigidly bound into the walls, or so that each floor shall not be free to expand or contract without involving injury or destruction to the walls themselves. The ideal to be approached should be that of a flat plate—the floor, however constructed, viewed as a whole—resting simply upon a flat ledge or projection inwards, or from the interior side of the walls, with a free space all round the plate to admit of its expansion, and the grasp of the plate upon the walls by friction to be nowhere such as to drag in with it the walls as the former contracts. Complicated forms in ground plan, in which architects so much delight, with curves and reëntering angles, should generally be avoided, and the forms of the chambers should consist of simple paralellopipeds; and the staircases would probably best be external and separate from the building, and divided at frequent intervals of the ascent by wrought iron doors, to cut off ascending draughts of air. Stone staircases, especially as ordinarily constructed, with each step built into the walls at one end, and the remainder overhanging, are wholly inadmissible; and perhaps the best hints for construction in detail might be derived from the spiral ascents or winding roadways available for carts and mules, and having no steps at all, but merely being

twisted planes, which exist in the great church of St. Peter's at Rome, and give access to its vast roofs. The total height above the ground of the building should not be excessive—perhaps three stories reaches the limit in buildings intended to be stored with merchandise; stores of eight or more stories in height, such as are to be found at London, Liverpool, Marseilles, and many other great ports, would tear themselves to pieces, no matter how constructed, as the effect of a persistent conflagration in any of the lower storeis. Lime, mortar, or any analogous cement destructible by a red heat, is inadmissible, and perhaps the most available cements will be found in siliceous or silico-ferruginous powders mixed with soluble alkaline or with earthy silicates into a plastic mass; these will adhere strongly to almost any brick or stone. Much might be said, too, as to how, and with precautions, fireproof buildings should be treated and managed. If nitrate of soda in bags, rough brimstone in heaps, and coffee beans or oil seeds in bags, are to be stowed away in the same loft, as may be often observed, the utmost perfection conceivable in fireproof construction is useless and thrown away. We might as well expect a magazine filled with gunpowder to withstand the effects of its ignition.

Space, however, forbids our pursuing the subject further here.

THE NEW THEORIES OF THE MOTION OF FLOWING WATER.

By Dr. G. HAGEN, Director of the Engineering Service of Prussia.

Translated, for Van Nostrand's Magazine, from "Zeitschrift für Bauwesen" for 1868,

By Mansfield Merriman, Ph. D., New Haven, Conn.

In the year 1850 the Congress of the United States ordered an exact investigation to be made of the regimen of the Mississippi River, in order to be able to prevent with some certainty the devastations which had so often previously happened in times of flood. The two engineer officers detailed for this purpose, Capt. A. A. Humphreys and Lieut. H. L. Abbot, published in 1861 the important results of this investigation under the title "Report upon the Physics and Hydraulics of the Mississippi River." The observations there communicated are without doubt highly important for river engineering, and they contain also many facts which may serve to clear up the mechanical relations existing in the flow of large masses of water. The authors have also endeavored to deduce therefrom general laws, and these have here in Germany not only excited great attention, but have, since the appearance of the translation, been regarded for the most part as completely proved. This translation, comprising the most important chapters of the Report with several Appendixes, was made by H. Grebenau, a Bavarian engineer officer, and was published at Munich in 1867 under the title "Theorie der Bewegung des Wassers in Flüssen and Canälen."

I have only lately succeeded in obtaining a copy of the original Report, and as my perusal of it has not, as I had hoped, cleared up the many doubts which pressed upon me in reading the translation, it appears to me necessary to subject the method by which these laws were found, and endeavored to be substantiated, to an open criticism. But if thereby these supposed theories are not to be accepted as such, no doubt therefore arises against the observations themselves; but we may rather conclude, from the lack of agreement between them and those theories, that they have been communicated complete and unchanged. It also cannot fail to be recognized, from the description of the methods of measurement, that they were carried out with expertness and judgment. Undoubtedly they belong among the best hydraulic measurements which we possess, and the fact that they were made upon so great a river as the Mississippi, gives them a yet higher value. It is however to be regretted, that the ob-

servations were not directly given, but are only stated by averages; also that some details were left undescribed, which were by no means unimportant. Nevertheless they contain as a whole very important data, which promise to give some disclosure concerning the hitherto so little known laws of the motion of water in river beds. I have begun an investigation of this kind, and in case of success will publish my results at some future time.

I. THE VARIATION OF VELOCITY AT DIFFERENT DEPTHS.

On pages 230–232 of the original (pages 56–63 of the translation) the authors record the observations which they have used in deducing the law of the variation of velocity at different depths, for the same point of a stream under a constant flow. The single observations are however not given, but with one exception, 2, 4, 8, 9, 10 or 16 measurements are combined. The tables contain only the averages of these combinations, and they consist of six groups, the first three of which give the observations made at high stages of water, and the others, those made at low stages.

Group I refers to a water-depth of 110 feet, and contains in thirteen series the averages of the velocities resulting from seventy-two series of observations, each made at nine different depths. Group II has a depth of seventy feet, and gives in eight series the forty-two velocities measured at seven different depths. In group III the water is fifty-five feet deep, and there are four series resulting from twenty-eight measurements, each made at six different depths. Group IV—water-depth 100 feet,—has five series of averages from twenty-four observations, at fifteen different depths. Group V—water-depth eighty feet—contains three series of twenty observations, at thirteen different depths. Lastly, group VI has a water-depth of sixty feet, and contains six series of thirty-six observations, each made for eight or nine different depths. There are hence communicated in these six groups and thirty-nine series of averages the results of 222 series of observations. The authors expressly mention that they have deduced from the latter the law of the variation of velocity at different depths.

The velocity measurements were made by establishing on shore two cross-section lines 200 feet apart, and observing the passage of floats with theodolites. The floats consisted of kegs fifteen inches high and ten inches in diameter, which were so loaded as to sink and remain upright. They were connected with surface floats of cork about eight inches square and three inches thick, submerged an inch and a half. The connecting cord was of the proper length so that the keg floated at the depth where the velocity was to be measured. This apparatus was placed in still water on a very windy day, and it remained immovable, from which it was concluded, that it was not only free from wind influence, but that it actually gave the velocity of the water at the depth of the keg. In perfect strictness this is not the case, and it would have been more suitable to give to the upper float the form of a boat, and to so fasten the cord that it should always keep parallel to the current, in order to have the least possible resistance.

This apparatus was thrown into the water from a boat lying at anchor at a suitable distance above the upper section-line, so that it should acquire the velocity of the water before the beginning of the observation. The time of passage past the upper section-line was noted by an observer with a theodolite, who then followed the float till it passed the second line. The passage of this was noted in a similar way by a second observer, who also indicated to the first the time by a loud shout, and the latter then measured the horizontal angle made by the float with the base line. In this way the distance of the float from the bank or the base was determined. The boat remained at the same spot while the floats were sent out for the different depths, but the order of observing was continually varied in every possible manner, in order to counteract, as far as possible, any change in the flow of the river then occurring.

The places in the river where the observations were made, or different bases measured, are only partly, and even then very incompletely described. We cannot learn whether the given depths of water continued for some distance or whether deep pools existed. The ob-

servations given by averages refer twenty-seven times to the place where the "prime base" was measured. This, according to a small sketch given in Fig. 2, Plate III, is situated near Carrollton, just below a very sharp bend of the river. The same drawing also shows that the "Race course base," for which four series are given, lies lower down the stream where it is very straight. Of the three other places of observation, at the "Locks base," the "Baton-Rouge upper and lower bases" nothing whatever is stated. The breadth of the river at these places is also nowhere given, although the distance of the float from the base is always recorded. According to the small sketch, the Mississippi is about 2,200 feet wide at the first base and 2,500 feet at the second.

In these tables the velocities are recorded to the ten-thousandth part of a foot, or accurately to the 30th part of a millimeter. Such exaggeration may influence favorably those readers who do not understand the matter, but in general it only shows that the investigations were carried on with reference to the degree of precision attainable. The two section-lines were 200 feet apart, and the velocities were found by dividing this distance by the number of seconds which the floats occupied in passing from the upper line to the lower. Conversely if we compute these times from the given velocities, we find, for example, from the first series of group I, 30, 30, 30, 30, 30, 31, 32, 34 and 38 seconds—every time a whole number of seconds. To measure halves or tenths of seconds is more difficult, and when, as here, one observer measures the beginning and another the end of the phenomena, such divisions must remain inaccurate, especially when the time is also to be marked by signals or shouts. Hence in these measurements half of a second is not to be relied on, or the probable error is equal to half a second at least. For this case then it is an even wager that the error in the computed velocity is not greater than $\frac{1}{10}$, that is, not greater than 0.1 feet. The first decimal place can hence not be regarded as accurate, while the authors give four.

It is still more remarkable that we find commonly only entire seconds in those cases where the given velocities are the averages of two series of observations. This happens for all the depths in series 2, 3, 12, and 13 of group I, and in the first and last series of groups II and III. The two observations must hence have always had the same number of seconds, and this in itself is not probable. Further, it seems strange that, without any particular reason being given for it, at all the depths in some series the velocities are only expressed by two decimals, cyphers being introduced for the last two; this happens in series 6, 7 and 8 of group I, and in series 3 and 5 of group II. Lastly it may be mentioned that in five series of group II, comprising thirty-six series of observations, the depth of water is given as sixty-five feet, while velocities are said to have been measured at a depth of sixty-six feet.

These are the 222 series of observations, from which the authors have deduced a very simple law for the variation of velocity at different depths, that with incredible precision agrees with the averages of the observations. The result is that the velocity-scale is a common parabola whose axis is horizontal and at a depth $0.297D$ below the surface, D representing the depth of water. The velocity v for any distance d from the axis of the parabola, is given by the expression

$$v = 3.2611 - 0.79222d^2,$$

in which d is taken in parts of D. The values of v computed from this formula for $0.1\ D$, $0.2\ D$, etc., were compared with the mean values from the 222 series of observations and an agreement within a few thousandths of a foot found for each point. The greatest difference is 0.0066 feet. The square root of the mean of the squares of all these differences is 0.0033, and hence the probable error is only 0.0023 feet. This accordance surpasses so greatly the imaginable limit of precision, that it not so much confirms the result, as renders it suspicious in the highest degree. The method by which the law was found and compared with the observations is described on pages 230–234 of the original (pages 57–65 of the translation).

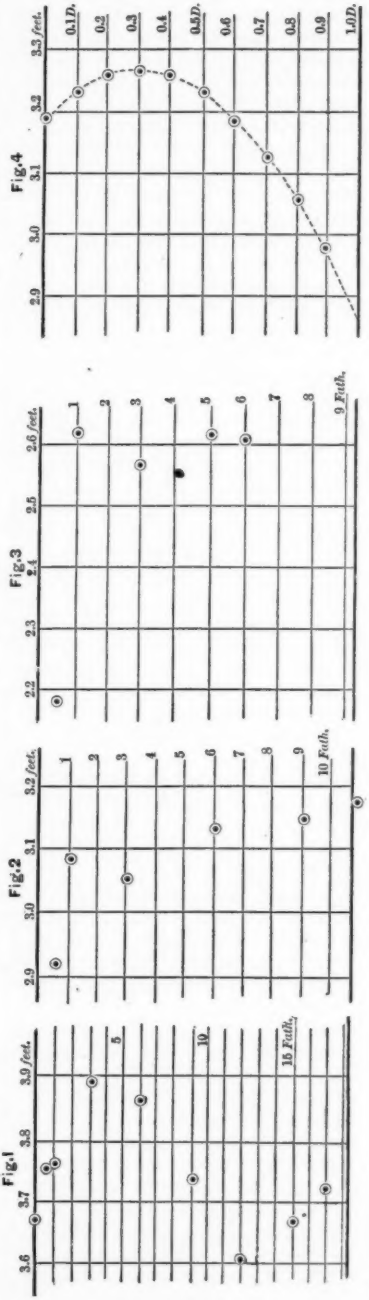
In this method,—the conviction having been first arrived at that the velocity decreased from the surface downward and then again decreased

toward the bottom—each single series of observations was plotted on so large a scale that thousandths of a foot of velocity were distinguishable. The drawing had hence a much greater precision than the measurements which it represented. Between these points a curve was then drawn which conformed to the above condition and agreed the most closely with the observations.

The reader cannot repeat this process, since only a single series of observations, separate from others, has been communicated to him. For this series a curve may be drawn quite agreeing with the above requirement. The other series are, however, combined and only the averages are given. In this combination the errors have already been partly eliminated, and the curves, which may be drawn for them, must correspond much closer to the true law than in the single series; but, nevertheless, they exhibit excessive anomalies which make every trial of this kind seem entirely arbitrary. If the law of the curve were already known the most probable values of its constants might be accurately found by the Method of Least Squares; but here it might easily happen that any other curve or even a straight line would result as more probable than that assumed. These plots, however, served only for the discovery of the curve, and hence nothing remained but to draw it by eye. What might be done by such drawings may be easily seen if we represent graphically some of the recorded series.

For example, Fig. 1 represents series 4 of group I, Fig. 2 the series 2 of group II, and Fig. 3, the series 3 of group III. These belong indeed to the most irregular, but they are already the result of the combination of 10, 8 and 16 single series of observations. In these last the anomalies must be still greater. But although even in these combinations such great deviations occur,—for example, in Fig. 2 the velocity continually increases with the depth to a point below the bottom of the river,—the mean values of all 222 series of observations agree with the parabola shown in Fig. 4 to such a precision that no deviation would be perceptible upon a much larger scale.

In explanation of these figures it must



be mentioned that the depths, in fathoms of 6 feet, are represented by the vertical ordinates, and the velocities in feet by the horizontal abscissas. If the authors selected for the graphical representation a scale fifty times as large as that here used, the difficulty of drawing the curves would certainly not be lessened, but rather increased.

Another way of treating the observations must here be alluded to. It is required to know the velocities for each series not merely at the surface but also at the depths $0.1D$, $0.2D$, and so on to $0.9D$, D being the total depth of the river. For this not only the sketched curves were used, but the values sought "were found by the most accurate interpolation that could be made." One cannot understand how an interpolation can be made between given quantities which exhibit such irregularities as in the case before us. The authors however succeeded so completely that the averages exhibit a fabulous harmony.

In order to find the kind of curve given by these averages the authors assume (as Dubuat did in the last century for another problem) that it must be a conic section. A comparison of the upper and lower branches showed that the axis lay very nearly at a depth $0.3\ D$ below the surface. Then each two of the computed velocities were used to find the coefficients of the two terms which contain the first and second powers of the abscissa (the vertex being the origin of coördinates), and it appeared that the coefficient of x^2 was very near zero, and hence the curve a parabola. The problem then was, to find that parabola agreeing best with the observations. Three constants were to be determined, namely, the abscissa and ordinate of the vertex and the parameter, the axis being assumed as horizontal. The Method of Least Squares, by which this problem may be accurately solved, was entirely unknown to the authors; they used therefore another graphical process and drew upon transparent paper many parabolas of different parameters. These were laid upon the points plotted from the averages of the observations, and in this way the best parabola and its best position determined. And here it is said that "after a little experience it is seldom necessary to make more than one or two trials." The agreement with the computed velocities is, as already remarked, excessive; it would, however, have been greater if a methodical process had been employed, for the differences although small are very regular, and may hence be lessened by a better choice of the constants.

The question remains to be answered, how this accordance of the new law with the observations above pointed out was caused. That the errors should have annulled themselves so completely by chance, is certainly not to be thought of, for the probability of it is far too small. From the thirty-nine recorded series, composed with one exception of many series of observations where the probable error has already been diminished in the ratio of the square root of the number of series, it results that the probable error of a single observation is greater than 0.1 feet. The assumption of this as its value is perfectly justifiable for velocities of 2 feet or over, for periodic oscillations prevail in rivers of a strong flow which exert great influence upon the measurement. If the probable error of a single observation is 0.1 feet, that of the arithmetic mean of 222 observations is

$$\frac{0.1}{\sqrt{222}} = 0.0067 \text{ feet.}$$

But, according to the computations of the authors, we find this probable error as mentioned above, equal to

0.0023 feet.

The probability of so small an error is only $\frac{43}{957}$ or 0.045. And the probability that this very small error should occur ten times in succession, namely at each of the depths, is,

$$0.045^{10} = 0.000000\ 000000\ 0335.$$

We can hence wager thirty-billions against one that such an elimination of errors would not occur of itself. In order to obtain an idea of the extremely small value of this probability, let us consider that a certain event occurs once every second, and that it may take by chance a particular form whose probability is the same as that of the above adjustment of errors, then we could only expect this particular form to appear once in a million years.

This form has however appeared, and from the probability of other causes

having produced it, we can determine the probability of those causes themselves. One such might have been an intentional selection made from the data of observation, those being rejected which did not agree with the law assumed as true. This cause is in itself not improbable. Persons who are not accustomed to scientific accuracy, generally consider such a proceeding as allowable and perfectly justifiable. In the case before us where the measurements were carried on under official, and hence at least under formal control, it might not have been easy to reject single observations, against which there was no suspicion when taken in the field. Moreover if there had been such rejections, the series, which so apparently contradict the law, as those represented graphically above, would not have been communicated. Lastly, it must be pointed out that the rejection of some series of observations might lead to a moderate reduction of errors, but that through this alone the agreement actually obtained cannot be explained. The probability for it, although perhaps ten or even 100 times greater than the former, would remain so very small, that chance could not be regarded as the cause of the event.

There is, however, another and indeed a very plausible explanation of this agreement. Let one try to draw curves with the above given properties, for the series here graphically shown, and it will be evident that the operation is entirely arbitrary. The most different curves have equal validity. The curves may be altered throughout their whole extent, or in chosen parts, without introducing an error. In this way an easy way was offered of making the observations agree with any curve which had been previously assumed, and thus decreasing at pleasure the computed differences. There would indeed have been no difficulty in drawing the curves so as to make the averages of the observations agree with the computation to seven places of decimals, or even closer.

The hydraulic student is obliged to view many principles as true and proved which are really very doubtful, but never before has it been suggested to him that he should accept a demonstration like this and therein recognize an advance of science.

As the supposed law, according to which the velocity-scale for every point in the stream is a parabola, has been found deficient in proof, the further test of the consequences derived therefrom is unnecessary, and its modifications may also be passed over. And if the same agreement is here found with the averages of the observations, it may be explained in a similar way, namely; when the velocities were sought for at different depths than at those observed, opportunity was again offered to draw curves at pleasure. I pass over also the investigation concerning the variation of velocity at different distances from the shore, to take up the problem of mean velocity, so important in river engineering.

II. DEPENDENCE OF THE MEAN VELOCITY UPON THE SLOPE AND CROSS-SECTION OF THE RIVER.

The authors endeavor first (pages 300–301 of the original, 125–128 of the translation) to solve this problem theoretically. They lay great weight on the resistance between the surface of the water and the air, and introduce also other different modifications of the usual assumptions. They arrive by this at an expression which is materially different from those regarded as correct—although without proof—in Germany and France. According to these last the product of the mean hydraulic depth by the relative slope is in general proportional to the square of the mean velocity, and in the more precise investigations is taken equal to the sum of two terms, one containing the first power of the mean velocity and the other its square root. The last ex pression however does not accord with all observations so well as the first. The authors observed this also and hence endeavored (pages 310–313), without regarding that theoretical development, to deduce an expression agreeing satisfactorily with the measurements. The trial cost, as they expressly mention, "much labor," and led finally to the result that, disregarding the small terms that are of little importance, the square of the mean velocity is proportional to the product of the mean depth by the *square root of the slope*. The expresssion at which they arrived is, for large streams

$$\sqrt{v}=\left(225\frac{a}{p+w}\sqrt{s}\right)^{\frac{1}{4}}-0.0388$$

in which v denotes the mean velocity, a the area of the cross section, p the wetted perimeter, w the breadth of the river and s the slope. For a horizontal water surface, where $s=0$ there is hence a slight flow, and this only disappears for a certain slight slope. This anomaly may indeed be overlooked, since the formula is an empirical one. If we disregard the last unimportant member, the expression becomes

$$v^2=225\frac{a}{p+w}\sqrt{s}.$$

The authors now collect thirty complete observations in which the area and perimeter of the cross-section, the slope and the mean velocity have been measured, and show that their formula accords better with them than any other previously given. Of these observations, nineteen were made on American rivers and canals, two are taken from Dubuat's measurements, one was made by Watt on a very small canal, five by Kragenhof on different branches of the Rhine, one on the Tiber and two on the Neva.

The coincidence shown between these observations and the new formula is by no means too great, for the error in velocity is once found 0.8 feet, and the probable error is nearly three inches. But by assuming the more simple expression

$$v^2=kts^x,$$

in which t is the mean depth, x the unknown exponent of the slope and k a constant factor, I found from all of Dubuat's observations, from Woltmann's and the numerous ones made by Lahmeyer on the Weser, that for $x=1$, the sum of the squares of the errors was smaller than for $x=\frac{1}{2}$. According to the observations of Dubuat, this sum of the squares was greater for $x=\frac{1}{2}$ than for $x=1$ in the ratio of 5 to 2, for Woltmann's in the ratio of 3 to 1, and for those of Lahmeyer in the ratio of 2 to 1. These measurements do not therefore confirm the new formula, but those made on the great American stream, which conclusively point to $s^{\frac{1}{2}}$, certainly do. The matter is hence not yet clear, but we may perhaps assume that measurements made upon small and upon great rivers are connected in other ways than through the exponents of the slope. To this is to be added, that the first power of the slope is in itself far more probable than the root.

As far as the observations instituted in America are concerned, it is here again to be regretted that many important data have not been published, and that the details of the measurements remain almost entirely unknown. Nevertheless they are, in comparison with previous observations (which certainly leave much to be derived) of great importance, and in further investigations on the flow of water in rivers they will certainly not be disregarded.

SOFT STEEL AND INGOT IRON.

From the "London Mining Journal."

A PROCESS for the manufacture of so-called soft steel or ingot iron, containing as low a percentage of carbon as .10, or possibly lower, or (say) containing an amount of carbon ranging from .18 to .10—that is to say, a soft steel can be made at any notice containing any desired amount of carbon within the range mentioned, has been patented by Messrs. Harvey, of North Woolwich, who propose to produce a soft steel especially suitable for wire ropes, telegraph wires (where very low electrical resistance is required), cable wire, and wire of various kinds which have to stand great elongation, twist, breaking strains, or other tests. Also for engines and other forgings, nail-making, plates, sheets, hoops, and every purpose where metal is required superior to charcoal iron, and where greater tensile strength, ductility, and toughness are required than can be obtained from the best wrought-iron worked in the ordinary way. The furnace preferred for this manufacture is the well-known so-called "Siemen's re-

generative" open hearth steel melting furnace, which is heated by coal gas and atmospheric air; or other regenerative gas or reverberatory flat-bottomed furnace may be substituted, capable of supplying a heat sufficient to melt wrought-iron, and keep the said wrought-iron in a fluid or molten state for some time. A tap-hole is provided at the back of the furnace through which the finished metal is run out. The said metal may first be run into a ladle of requisite capacity as is usual, and from thence it is tapped into round, square, or other shaped moulds, as may be required to suit the purposes for which it is intended.

The improved process consists in charging on the bottom of the furnace (such furnace to have attained previously the highest temperature, or nearly so, which it can command) from 40 to 50, or (say) 45 per cent. (of total charge exclusive of ore) of pig-iron, the quality of which pig-iron shall be hereinafter specified, to which is added on charging 10 per cent. of ore, which ore will begin to oxidise the carbon contained in the pig-iron as soon as the latter melts, and if the pig-iron contains less manganese that 1.5 per cent., the manganese in the pig-iron must be made up by charging with the pig-iron from 1 to 4 per cent. of spiegeleisen containing 20 per cent. of manganese; thus, if the pig-iron contains about 1.5 per cent. of manganese no spiegeleisen is charged, but if it contain (say) only .5 per cent. of manganese then about 2 per cent. (of total charge exclusive of ore) of the said spiegeleisen should be charged. Different raw material is used according to the tests which it is intended that the finished metal should be best able to bear.

When the whole is melted a little more air is put on in order to assist in decarburising the metal, and the full heat is kept up until the ore has all worked off the surface of the metal, or until the ore has become reduced by the action of the carbon contained in the pig-iron. At this time the metal will be seen to sink below the former level, and after some time it will boil uniformly over the entire surface. A sample of the boiling metal should now be taken out, cooled in water, dried, and hammered on a smith's anvil; if on the first blow it breaks and flies 1 cwt. (about) of ore should be thrown in, which operation is repeated until the sample stands a blow on the anvil without fracture, thereby indicating that the metal contains from .60 to .75 per cent. of carbon. When this point is reached from 30 to 40 per cent. (of total charge, exclusive of ore) or (say) 35 per cent. of wrought-iron or soft-steel scrap is charged on the banks of the furnace, in small quantities at a time (say) 5 to 10 cwts. at either end, which wrought-iron, in whatever shape, is allowed to melt almost entirely by heat while still on the banks, without being pushed into the bath of metal. That portion, however, of the wrought-iron or soft steel scrap which is charged much above the level of the metal may with advantage be turned in when at a good white heat, in order to save waste by oxidation. The object of this mode of charging the wrought-iron or soft steel scrap is to prevent the cooling of the molten metal, which would probably result if the charges were introduced without previous heating, thereby incurring the risk of portions assuming a pasty condition, instead of being uniformly thin and well melted. Should the furnace, however, be working very hot, some of the wrought-iron or steel scrap may be turned in with advantage when at a good heat. During this melting in of the wrought-iron or steel scrap the flame should be kept bordering on a non-oxidising quality.

By running in the wrought-iron or soft steel scrap as described—at this period, it liquefies and renders very thin the thick scum or too silicious slag which accumulates on the surface of the molten pig-iron after being nearly decarburised by the ore, as is the case in working the so-called "pig and ore" process, thereby preventing the heat (gas and air) from penetrating through the metal, and so retarding the decarburising effect of the oxygen contained in the flame, and increasing the liability of the metal to assume a pasty condition and sink to the bottom by reason of the deficiency of heat; but after the wrought-iron or soft steel scrap is run in, the slag being in a very liquid state allows the heat and flame to penetrate perfectly, thereby preventing the metal from sinking to the bottom. Owing to the wrought-iron containing but little carbon, it is found

that when the whole of it is run in in the manner described, the said wrought-iron by its diluting action and the oxygen in the flame by its decarburising action (during the time the wrought-iron was melting) will have reduced the carbon contained in the metal to not more than .14 or .15, and sometimes even lower, thus reducing the carbon much quicker than by working with ore for the same object, as the time required for the melting in of the wrought-iron does not exceed 1½ hour, though it is possible to work it in less time than this.

When the whole of this wrought-iron or soft steel scrap has been melted and turned well in the bath of metal, a rabble is inserted to ensure a thorough admixture of the wrought-iron or steel scrap with the other metal. A piece of ore should now be thrown into the bath, and if after a minute or two the ore floats on the surface, and there appears to be no action between the metal and the ore, or the ore is only reduced with difficulty, this is an indication that nearly the whole of the carbon has been practically eliminated—that is to say, that the carbon contained in the metal has become so low that neither the ore nor the oxygen of the flame has any perceptible burning or taking out action on the carbon that remains; but if the ore disappears after two or three minutes, this is an indication that a little more ore is required, which is accordingly thrown in at intervals till there is no action, as above explained. When this is the case no more ore is required, but a full heat is kept up for fifteen or twenty minutes, so that the oxygen in the flame may possibly burn out a little more of the carbon, and the metal will thereby be considerably heated.

When nearly the whole of the carbon has been eliminated the surface of the bath of metal will show numerous thin and very liquid pools, like water, and on which the heat seems to have greater effect than on the remaining surface. A sample now taken out will be found to contain about .12 per cent. of carbon. When this point is reached wrought-iron ranging from ten to thirty per cent. or higher (of the total charge exclusive of ore) is introduced, according to the carbon required in the finished metal—that is to say, if soft steel containing .10 per cent. of carbon is required, then from twenty-three to thirty per cent. of wrought-iron must be added. If .15 per cent. of carbon is required, 17 per cent. of wrought-iron must be added, and so on, the quantity charged being in proportion to the carbon required. Such wrought-iron should be put in in quantities of five or six cwts. or more at a time on each bank, and allowed to melt in entirely by the heat of the furnace without pushing in any part thereof, except that which is furthest away from the metal, which should be turned into the bath at a white heat. In this way the metal will maintain its full liquidity and heat, and consequently it will not readily solidify when in the ladle or elsewhere after being tapped from the furnace.

In order to dilute or reduce the amount of the carbon contained in the metal soft wrought-iron must only be here used—that is to say, iron containing less than .12 per cent. of carbon, this latter being about the amount already in the metal; therefore, iron containing .11 or .10 per cent. of carbon or lower is used with advantage for this object, and if a soft steel be required with carbon as low as .10 per cent. it is found best to use iron with nearly all the carbon worked out, or containing (say) .05 per cent., or lower, as then a less quantity can be charged than if the iron contained more carbon. The carbon contained in the whole may thus be reduced to .06 or .07 before charging the ferro-manganese, which added to the small amount of carbon put in by the ferro-manganese will bring it to .10 per cent. in the finished metal. If the carbon contained in the metal before charging in the ferro-manganese should have been reduced to .12 or .15 per cent. by the first addition of wrought-iron, and the amount of carbon in the finished material be required to be not less than .17 or .18 per cent., then the amount or a little more of the ferro-manganese can be charged without putting in the second lot of wrought-iron. The quantity of ferro-manganese (say .7 per cent.), as stated below, will be found amply sufficient, provided the quantity of manganese in the pig-iron charged at first be made up.

When the wrought-iron in this last operation has disappeared from the banks, or has become dissolved by the

molten metal, a rabble or bar should be frequently inserted and stirred about in order to assist in bringing the same into close contact with the other metal, thereby rendering it perfectly liquid and uniform with the other existing metal. This rabbling should be renewed at intervals during half an hour after the wrought-iron has disappeared from the banks, in order to bring the metal into contact with the full heat of the furnace before tapping it into the ladle. A sample will now be found to contain from .6 or .7 to .11 per cent. of carbon, according to the quantity of wrought-iron charged at the last operation, and according to the proportion of carbon contained in such wrought-iron. A little burnt lime (say) 15 or 20 lbs., is now thrown in to assist in keeping a fluid slag, and after ten minutes 7 per cent. (of the total amount charged exclusive of ore) of powdered ferro-manganese containing not less than 70 per cent. of manganese is now charged into the centre of the molten bath, which ferro-manganese will melt in about two or three minutes, but in order to secure a perfect melting and union from five to ten minutes should be allowed previous to tapping, and the metal should also be well rabbled before running out.

ENGINEERING NOTES.

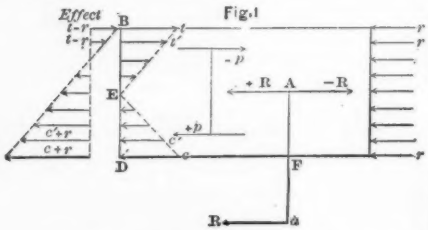
By Prof. WM. CAIN, A. M., C. E., Prof. of Mathematics and Engineering, Carolina Mil. Inst., Charlotte, N. C.

Written for Van Nostrand's Engineering Magazine.

1. DECOMPOSITION OF RESULTANT ON A CROSS SECTION.

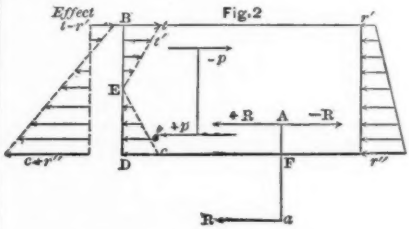
Conceive a slice BDA, Figs. 1 and 2, of width unity; the cross-section BD, perpendicular to the plane of the paper, being a *rectangle*.

Also suppose the resultant R of all outward forces on the cross section BD, to lie in a plane parallel to the paper, through the center of the slice and perpendicular to the supposed cross-section. Let us suppose *first* that the cross section can develop both *tensile and compressive* resistances as in a solid beam, and that R passes through *a without* the cross section.



If at some point A in the medial plane, we conceive two opposed forces +R, −R, parallel and equal to R, the force R with the force −R forms a right handed couple RR, that can be replaced by the equal couple $p\overline{p}$ or the forces, $t, t' \ldots c', c$, equal and opposed to the uniformly increasing tensile *resistances* from E to B and the compressive *resistances* from E to D, E lying in the center of gravity of the cross section.

2. In Fig. 2 A lies anywhere, in the medial plane, *within* the cross section (a further limit will presently be indicated); in Fig. 1, A lies in the center of gravity of the cross section, so that +R passes through E.



The force +R at A, Fig. 1, is decomposed into forces, $r, r \ldots$, *supposed* uniformly distributed. In Fig. 2, +R at A is *supposed* decomposed into $r', \ldots, r''$, straight lines limiting the arrows representing the forces.

3. The hypothesis that in an elastic beam the forces $r' \ldots r''$, Fig. 2, are proportional to the ordinates of a trapezoid

will be proved at art. 18. This has long been a favorite hypothesis, but I have not met with a demonstration anywhere, simple as it is.

4. In both Figs. 1 and 2, the sum of the moments of the forces $r \ldots$ about A must be zero in order that R may be their resultant.

5. Now the forces $r, \ldots$, add to some of the forces, $t', t', \ldots, c', c$, and subtract from others, thus leaving as the actual *forces*, opposed by equal tensile or compressive resistances at the cross section, $t-r, t'-r, \ldots, c'+r, c+r$, Fig. 1, or, $t-r', \ldots . c+r''$, Fig. 2, as shown to the left of the figures.

6. We shall now show that the resulting actual forces $t-r \ldots .$ by either mode of decomposition, Figs. 1 or 2 are the same.

For brevity put $BD=h$, $AF=y$ (Fig. 2) or $F=d$; then by the theory of the center of gravity (see Weisbach's Mechanics, Coxe, art. 110), since $+R$ acts at the center of gravity of the trapezoid $r' \ldots r''$ Fig. 2, and can be represented by that trapezoid, h being its height,

$$y=\frac{r''+2r'}{r''+r'}\frac{h}{3}, \text{ and } R=\frac{r'+r''}{2}h$$

hence, by elimination,

$$r'=\frac{2R}{h}\left(\frac{3y}{h}-1\right). \ldots (1); \quad r''=\frac{R}{h^3}(4h-6y) \qquad (2)$$

Now calling f the strain t or c, we know from the theory of flexure, that in Fig. 1.

Moment of R at a about $A=R(d+\frac{1}{2}h)=\frac{1}{6}fh^2$, whence

$$f=\frac{6R(d+\frac{1}{2}h)}{h^2}=t=c$$

Uniform compression $r=\frac{R}{h}$,

Hence,

$$(t-r)=\frac{R}{h}\left(\frac{6(d+\frac{1}{2}h)}{h}-1\right)=\frac{R}{h^2}(6d+2h) \quad (3)$$

$$(c+r)=\frac{R}{h}\left(\frac{6(d+\frac{1}{2}h)}{h}+1\right)=\frac{R}{h^2}(6d+4h) \quad (4)$$

In Fig. 2,

the moment $R\overline{R}=R(y+d)=\frac{1}{6}fh^2$

$$\therefore f=\frac{6R(y+d)}{h^2}=t=c$$

Therefore, in Fig. 2,

$$(t-r'')=\frac{R}{h}\left(\frac{6(d+y)}{h}-\frac{6y-2h}{h}\right)$$

$$=\frac{R}{h^2}(6d+2h)$$

$$(c+r'')=\frac{R}{h^2}(6y+6d+4h-6y)$$

$$=\frac{R}{h^2}(6d+4h)$$

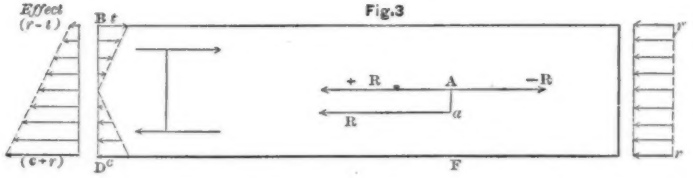
These values for the extreme forces being the same as for the corresponding forces $(t-r)$, $(c+r)$, eqs. (3) and (4), we conclude that either mode of decomposition leads to the same result.

7. From eq. (6), $r'=o$ when $3y=h$, hence if forces $r' \ldots r''$ are all to be compressive, A must not be supposed nearer edge than $\frac{h}{3}$.

8. If R at a is sufficiently near A, $t-r, t'-r, \ldots .$ may all act as compressive forces, thus not requiring any tensile resistances at the cross section BD as at a joint of the voussoir arch.

The effect is the same, as previously shown, if R at a is decomposed at once into the compressive forces $r' \ldots . r''$ of Fig. 2, equivalent to, $r-t, \ldots, c+r$, of Fig. 3, according to the ordinates of a trapezoid.

Fig. 3

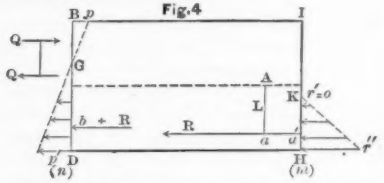


9. From eq. (3), $t-r=o$ when $d=\frac{1}{3}h$ or when $aA=\frac{1}{3}h$. When a is farther from A than $\frac{1}{3}h$, then $(t-r)$ is positive and we assume tensile *resistances* at B to oppose the forces.

But suppose the joint BD can offer no

tensile resistances, then it is wrong to decompose R as before, if by another method of decomposition stability is assumed. Many writers seemed to have overlooked the following method of decomposition entirely:

Fig. 4



R at a, Fig. 4, is the resultant in position on the joint BD. We may decompose R according to the ordinates of a triangle as at (m), if we assume the law of the trapezoid as before, r' is now o, hence $y=\frac{k}{3}$, where $k=\mathrm{KH}=$ height of trapezoid or triangle.

Placing

$$\overline{\mathrm{A}a}=\mathrm{L},\ \mathrm{HK}=\mathrm{K}=3\,\overline{\mathrm{H}a'}=3(\tfrac{1}{2}h-\mathrm{L}) \quad . \quad (5)$$

10. Let us now compare this result with the case shown at (n), where the beam is supposed to supply tensile resistances along BG. Call the resistances per square unit at B and D, p and p' respectively; *i. e.*, $p=t+r$ and $p'=c+r$.

From similar triangles, calling $\mathrm{DG}=x$,

$$p' : x :: p : h-x \therefore x=\frac{p'\,h}{p+p'}$$

Now substituting for p and p' their values, eqs. (3) and (4) and replacing $(d+\frac{1}{2}h)$ by L, we have,

$$x=\mathrm{DG}=\frac{\dfrac{\mathrm{R}}{h}\left(\dfrac{6\mathrm{L}}{h}+1\right)h}{\dfrac{\mathrm{R}}{h}\ \dfrac{6\mathrm{L}}{h}}\ \frac{h^2}{12\mathrm{L}}+\frac{h}{2} \quad . \quad (6)$$

Now from eqs. (5) and (6)

For $\mathrm{L}=\frac{1}{6}h$,	$\mathrm{HK}=h$,	$\mathrm{DG}=h$,
$\frac{1}{3}h$,	$\frac{1}{2}h$,	$\frac{3}{4}h$,
$\frac{5}{12}h$,	$\frac{1}{4}h$,	$\frac{7}{10}h$,
$\frac{1}{2}h$,	0,	$\frac{2}{3}h$,

That is, HK<DG, when L lies between $\frac{1}{6}h$ to $\frac{1}{2}h$; or the point of no strain in the beam lies nearer the edge DH where the joint can oppose no tensile resistances than when we suppose them exerted.

11. Let us now compare the strains p' and r''.

In eq. (2), when KH=K<BD, then we must write K=height of trapezoid, for h; we also have $\mathrm{K}=3\overline{\mathrm{H}a'}=3y$, Fig. 4. Again, moment of R at a about $\mathrm{A}=\mathrm{R}(\frac{1}{2}h-y)$, &c. See art. 6. Hence eqs. (2) and (4),

$$r''=\frac{\mathrm{R}}{9y^2}(12y-6y)=\frac{2}{3}\frac{\mathrm{R}}{y} \quad . \quad . \quad (7)$$

$$p'=(c+r)=\frac{\mathrm{R}}{h^2}\ (4h-6y) \quad . \quad . \quad (8)$$

Thus for any values of y between 0 and $\frac{1}{3}h$ (see table) we find that, $r''>p'$, always.

y	r''	p'
$\frac{1}{12}h$	8	$3\frac{1}{2}$
$\frac{1}{6}h$	4	3
$\frac{1}{4}h$	$\frac{8}{3}$	$\frac{5}{2}$
$\frac{1}{3}h$	2	2
	$\frac{\mathrm{R}}{h}$	$\frac{\mathrm{R}}{h}$

There is therefore greater compression at edge DH when the real forces are as at (m), than when the beam can oppose tensile resistances as at (n). Also since KH<DG, within the same limits ($y=o$ and $\frac{1}{3}h$), the compression is more uniformly distributed at (n) than at (m); hence the beam will bend more when, as in the voussoir arch, a joint BD can only oppose compressive resistances, than when, as in a solid beam, tensile resistances can be exerted. As a consequence of this compression at lower edge, there being none (according to our hypothesis —the law of the trapezoid) at K in case (m), the joint above K must open, and could in fact be removed without interfering with the distribution of the forces at all. While it is not easy of comprehension, that on an incompressible joint every part should not bear some part of R, yet on real compressible joints it would be hard to conceive of the joint *not* opening above some point K. Experiment sustains this view. Some writers have erroneously asserted that the opening was due to tensile forces. It may be observed, that if, as in the voussoir arch, these tensile forces are unbalanced rupture must ensue.

12. *The decomposition of* R *cannot be the same for an open joint as for a solid*

beam, when L *lies between* $\frac{1}{6}h$ *and* $\frac{1}{2}h$, for if we assume for a solid beam the disposition (*m*), the edge nearest *R* is most compressed, the joint would tend to open above K; but as there are tensile resistances there that prevent it, the disposition (*m*) is not correct for the solid beam, hence some other disposition as (*n*) is correct.

13. In experiments by the writer with wooden voussoir arches—gothic being 14 *in span*, 12.12 *in rise* and weighing 4.16 lbs., the resultants at the joint of rupture approached within $\frac{1}{15}h=\frac{1}{15}$ depth joint from edge without rupture ensuing (Cain's Voussoir Arches, p. 85). They likewise indicate that the nearest approach of "*a*" (Fig. 4), to the edge consistent with stability, called its "limiting position," for a plane joint probably depends upon the magnitude of R (as we should infer, *r*′ increasing with R, whilst KH is constantly 3.H$\overline{a'}$), and the special compressibility of the material. Of course "*a*" cannot be upon the very edge, since R must act upon a finite surface.

In the experiments, the joints generally opened on the side farthest from the resultant, before the final weight was applied, and remained open, bearing at the edges only, thus slightly distorting the arch. As in the investigations of stability, the arch was assumed non-deformable, the resultant passed nearer the edges than estimated.

14. The spandrels of a stone bridge, by exerting horizontal forces at the haunches, may keep the joints entirely or partially closed. The disposition of forces then may, in a bridge with solidly built spandrels approximate to (*n*), Fig. 4.

15. In the disposition (*n*), Fig. 4, R is transported from *a* to *b* nearer G, thus R at *b* and couple Q Q (shaded forces) being just equivalent to R at *a*. In fact, conceive R at *b* and couple $\overline{QQ}$. Now at *a* conceive +R and −R applied. Now R at *b* with −R at *a* form a couple which must just equal $\overline{QQ}$ leaving +R at *a* as the resultant of R at *b* and $\overline{QQ}$. Also, since $\overline{QQ}$ is right handed, $\overline{R\,R}$ must be left handed, hence *b* is nearer G than *a*.

The total strain of compression on DG is greater than on HK at (*m*), but since $p'<r''$ and KH<GD, the strain is more equalized on DG than on KH, and (*n*) is the most advantageous disposition of forces.

16. Some writers assert that if A*a* is greater than $\frac{1}{6}h$ and tensile resistances are not supplied at B, the arch must fall. The experiments mentioned have proved finally that if R lies between $\frac{1}{6}h$ from center and its "limiting position," very near edge, that rupture does not ensue; hence there are no tensile forces exerted, hence R is simply decomposed into compressive forces. It may be remarked that the resultant will at the joints of rupture in an arch reach its limiting position, whether the theorist wishes it or not. The *true line* of pressures most likely never keeps within the "middle third" in any actual arch—certainly not for the smaller spans with their joints.

For any other continuous symmetrical form of cross section than the rectangular, we can by parallel planes divide it into strips that are approximately rectangles. As the error of so considering them diminishes indefinitely with the width of the strips, we can assume the previous conclusions for any continuous symmetrical form of cross section, since the error can be made as small as we please, becoming 0 at the limit.

18. As to the assumed law of the trapezoid, we may state generally *for a rectangular form of cross-section, that if* R *is on one side of the neutral axis, the beam will suffer greater compression on that side, hence bending will occur.*

To prove it, we premise that when bending occurs the extensions or compressions of the fibers, and hence the forces *per square unit* acting on them, must necessarily be directly as their distance from the neutral axis. [G in Fig. 4 (*n*), or K in Fig. 4 (*m*)].

Where tensile resistances are exerted at one side and compressive at the other as in Figs. 1, 2, or 4 (*n*) flexure or *bending* must occur.

Now consider the resultant R, in the cross section as at Figs. 3 and 4 (*m*). There are three suppositions:

1*st*. In Fig. 4 (*m*) call $r' \ldots r''$, the compressive forces *per square unit* and R their resultant. Suppose an *equal shortening of the fibers* due to the force R at *a*′ on $\overline{Ha'}$ and $\overline{a'K}$. This requires a uniform distribution of the component

of R ($r' \ldots r''$) on KH, whence $Ka' = Ha'$.

But as the fibers give equally on KH, KI must also suffer an equal shortening of the fibers, but R at a' cannot be the resultant of *uniform* forces distributed over HI, hence the first assumption is incorrect.

2*d*. Suppose *the actual shortening of the fibers on* $\overline{Ka'}$ *greater than on* $\overline{Ha'}$.

This can *only* happen by supposing the beam *more compressed at* I *than at* H; thus bending occurs and the forces $r' \ldots . r''$ must *regularly* decrease from I. But in that case R cannot be their resultant when a' is below the neutral axis. Our 2d. surpposition is thus false.

3*d. and last. Suppose the actual shortening of the fibers greater on* $\overline{Ha'}$. As before, bending occurs and the forces $r' \ldots . r''$, consequently, regularly increase from K to H according to the law of the trapezoid. In this case R can be and is their resultant. There is no inconsistency here with the supposition.

The first two cases being untrue, the last, the only remaining one, is true.

We have thus proved that bending always occurs when R is not in the center of gravity of the section; and that in a rectangular cross-section, that can oppose no tensile resistances, the resultant R is decomposed according to the ordinates of a trapezoid, whence follows the conclusions of previous articles.

19. The reasoning and conclusions of the last article are easily extended to continuous forms of cross section, symmetrical about the neutral axis, as in art. 17.

20. For any form of cross-section, if we admit that the beam *bends* when R is not in the neutral axis, the extensions and compressions must be as their distance from the neutral axis, *i.e.*, the tensile or compressive force per square unit on a fibre is directly as the distance from the point of no strain.

21. The writer has thought proper to endeavor to put in a clear and simple manner the foregoing, both to meet enquiries as well as misapprehensions. In VAN NOSTRAND'S MAGAZINE for Sept., 1877, p. 257, is an article "On the Strength of Columns," by E. Hatzel, Translated, &c. The following articles on the same subject embody the same steps followed by Mr. Hatzel, with two important exceptions that will be noted further on, thus reaching a formula slightly different from his.

22. STRENGTH OF LONG POSTS.

Let ABCD represent the neutral axis of a pillar bent by the force P at A acting in the direction AD.

Call the abscissa $AF = x$, ordinate $BF = y$ $CE = c$, $ACD = AED = l$, $\delta =$ radius of curvature at B, $A =$ cross-section at B when $y = BF$.

I = moment of inertia of cross-section about neutral axis,

E = modulus of elasticity,

f = maximum resistance per square unit of A.

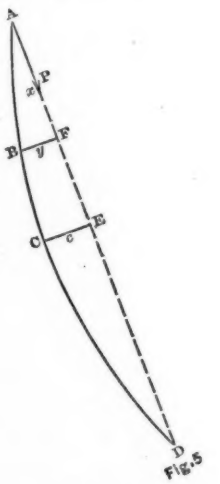


FIG. 5

Let us suppose, as in Fig. 1, two opposed forces at B, each equal and parallel to P; then, as in Fig. 1, the compression per square unit due to +P is,

$$r = \frac{P}{A} \quad . \quad . \quad . \quad . \quad (1)$$

Now the moment Py can only be resisted by a moment (this seems to have been overlooked by Mr. Hatzel as he takes the moment of inertia about the edge of the cross-section, which gives no resisting *couple* at all): hence calling (Fig. 1) $t = c = f_1 =$ max. strain per square unit, at a distance from the neutral axis $= g$, we have from the well known theory of flexure,

$$M=Py=\frac{f'I}{g} \quad . \quad . \quad . \quad (2)$$

[It may be observed that from (1) and (2) we may find $r+f'=c+r$ and $f'-r=t-r$ of Fig. 1 for any cross-section, M being the moment $R(d+\frac{1}{2}h)$].

From (1) and (2), we have for the stretching per unit of length

$$\frac{r}{e}=\frac{P}{Ae}, \quad \frac{f'}{e}=\frac{g}{EI}Py$$

The sum of which represents the greatest admissible shortening, $\frac{f}{E}$, if P represents the crippling weight, and we put $y=c$ (its max. value

$$\therefore \frac{f}{e}=\frac{P}{AE}+\frac{g}{IE}Pc$$

$$\therefore fA=P\left(1+\frac{gA}{I}c\right). \quad . \quad (3)$$

The equation of the elastic line ACD (see Weisbach, art. 265), is

$$y=c \sin.\left(x\sqrt{\frac{P}{EI}}\right)$$

When $x=\frac{1}{2}l$, $y=c$

$$\therefore 1=\sin.\left(\tfrac{1}{2}l\sqrt{\frac{P}{EI}}\right) \quad \therefore \tfrac{1}{2}\pi=\tfrac{1}{2}l\sqrt{\frac{P}{EI}}$$

$$\therefore \frac{EI}{P}=\frac{l^2}{\pi^2}$$

Now at c,

$$\delta=\frac{EI}{Pc}=\frac{l^2}{\pi^2 c} \quad . \quad . \quad . \quad (4)$$

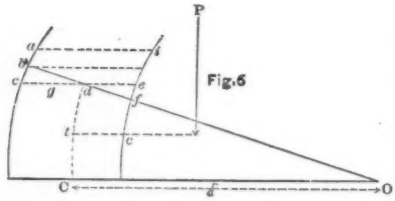


Fig. 6

In the accompanying figure $OC=\delta$, $Cd=1$, $dc=g$. The cross section gi parallel to CO is moved to bf. bc=extension of outermost fibre due to moment $Py=k$. Or if compression $ef>bc$ due to same moment, $k=ef$ and $de=g$.

The uniform compression due to P is $ac=ie$. When $ac<bc$ there is no tensile strain.

In any case we have the proportion,

$$\delta : 1 :: g : k \quad \therefore \delta=\frac{g}{k}$$

This gives in eq. (4)

$$\frac{g}{k}=\frac{l^2}{\pi^2 c} \quad \therefore c=\frac{kl^2}{g\pi^2}$$

which, substituted in (3), gives

$$P=\frac{fA}{1+\frac{kl^2}{\pi^2}\frac{A}{I}}$$

But $\frac{A}{I}=\frac{1}{r^2}$, where r=radius of gyration of the cross-section about the neutral axis. Also for brevity write, $\frac{k}{\pi^2}=a$, and we have the usual formula,

$$P=\frac{fA}{1+a\left(\frac{l}{r}\right)^2} \quad . \quad . \quad . \quad (5)$$

[Remark. k has been assumed by Mr. Hatzel as identical with "λ=greatest possible shortening which the material can sustain per unit of length," whereas $f\,i$ in last figure=λ].

23. In this formula, (5), $a=\frac{k}{\pi^2}$ would be known if k were known, but as k depends upon the unknown moment, Pc (c being unknown) it is obviously impossible to determine it except by experiment. "a", by theory, is the same for any cross section. It seems reasonable to suppose that a increases with l, as the bending (which determines $a=\frac{k}{\pi^2}$) is probably greater the longer the pillar.

A comparison of numerous experiments does not support this view.

"a" is usually assumed constant.

In the experiments on pillars made by the Cincinnati Southern Railway, "a" was assumed constant and "f" computed. It was found that for the same shapes and iron f as computed does not vary more than the tensile strengths of iron of the same manufacture. For any special shape it is recommended (see Mr. T. D. Lovett's report to C. S. R. 1875) to find f by experiment, when the above formula is applicable.

"For swelled columns the formulæ are are also applicable, provided the diameter at the end be used." "Riveting at the ends and tail bearings are of great importance."

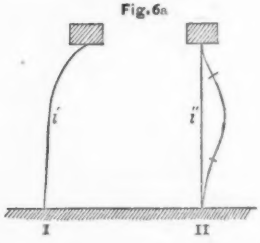
24. In Fig. 6*a* (I), *for pillar fixed at one end and round at the other*, formula (5) applies if we replace l by $2l'$,

$$\therefore P = \frac{fA}{1+4a\left(\frac{l'}{r}\right)^2} \quad . \quad . \quad (6)$$

25. In Fig. 6*a* (II) *column fixed or flat at both ends*, the curvature of Fig. 7 (I) is repeated four times. For l' then in (6) put $\left(\frac{l''}{4}\right)$

$$\therefore P = \frac{fA}{1+\frac{a}{4}\left(\frac{l''}{r}\right)^2} \quad . \quad . \quad (7)$$

Fig. 6a



26. Dr. Rankine says that Hodgkinson verified the relations expressed by eqs. (5) and (7), but "found the strength of a pillar fixed at one end and rounded at the other, to be a mean between the strength of two pillars of the same length and diameter, one fixed at both ends, and the other rounded at both ends."

The constant $\left(\frac{a}{4}\right)$ in eq. (7) is given by Rankine and others at $\frac{1}{36000}$; and for columns *hinged on pins at the ends*, it is recommended by American engineers that we put $\left(\frac{a}{4}\right)$, in eq. 7 $=\frac{9}{36000}$, f being determined by experiments on hinged joints.

27. It may be observed that when, for rounded ends $a\left(\frac{l}{r}\right)^2 = 1$, (the value of K corresponding to P being substituted) that some part of the cross-section is in tension, see eqs. (1), (2) and (3). This should be avoided for cast iron. If the pillar cannot exert tensile forces, it will bend, but will not break in two until P is at its "limiting position," very near the edge (see arts. 9 to 14). Compare the magazine article quoted, p. 261 "5th."

28. It is advisable in stone or brick pillars from a practical (not theoretical) point of view, that $a\frac{l^2}{r^2} < 1$. For when $a\frac{l^2}{r^2} > 1$, if the mortar is cohesive, tensile resistances will be exerted until the cohesion is destroyed, when the joint opens and there being only compressive resistances, the pillar bends more and of course P approaches its limiting position."

It is certainly improper from a practical point of view, to have the joints of a vertical pillar opening. In a voussoir arch you cannot prevent the resultant on the joints of rupture passing as near the edge as its limiting position, though the spandrels may prevent opening of the joints. It is reported that in many bridges the voussoirs bear on the edges only.

29. Mr. Hatzel found from some experiments on brick piers that when $l = 12$ diameters, tensile forces might be feared.

30. It is usual to take a certain proportion of the crippling weight for the safe load a pillar can bear. It has been recommended by a Committee of the American Society of Civil Engineers, appointed to propose "Means for Averting Bridge Accidents," (see VAN NOSTRAND'S MAGAZINE, Oct. 1875, p. 306), not to exceed for wrought iron in compression the following strains per. square inch in lbs. for square or cylindrical sections,

Diameters.	Pounds per Square Inch.	
	Square ends.	Round Ends.
10	10,000	7,000
10 to 15	9,000	6,500
15 to 20	8,000	6,000
20 to 25	7,500	5,500
25 to 30	6,800	5,000
30 to 35	6,000	4,000
35 to 40	5,000	3,500
40 to 50	3.800	2,500
50 to 60	3,000	2,000

On the Committee were some of the first engineers in this country.

After the Ashtabula accident, the same

limit of strain was proposed to the Ohio Legislature.

31. In this table the safe strains allowed diminish more rapidly with the height than the formula warrants. Hence it has been proposed to tack on a variable factor of safety. Thus, if d= diameter of column in the direction of the bending we may write for *flat ends*, he safe strain per sq. in.

$$P = \frac{1}{4+\frac{\frac{l''}{d}-15}{10}} \cdot \frac{fA}{1+\frac{a}{4}\left(\frac{l''}{r}\right)^2} \quad . \quad (8)$$

If we put f=38500, A=1 and $\left(\frac{a}{4}\right) = \frac{1}{36000}$, we find for $\frac{l''}{d} = 10$, P = 10761; $\frac{l''}{d}$=27, P=6371; $\frac{l''}{d} = 45$, P = 3793, which agrees tolerably well with the table.

The above recommendations are to be amended from time to time as experience may suggest.

32. In these formulæ as in most specifications, the safe strain allowed for tension or compression is usually some constant fraction of the breaking weight, irrespective of the extremes of load to which the number may be subjected.

Since the experiments of Wohler and Spangenberger and the publication of the treatise of Weyrauch ("Constructions of Iron and Steel") it may pertinently be asked, even if we cannot accept Weyrauch's formulae for *bridge members* whether our own empirical formulae cannot be very much improved upon in the light of the experiments mentioned.

Wohler's law is a fixed fact and should not be ignored.

The suggestions thrown out on this subject are of course open to much criticism, as no experiments can be appealed to for numerical values, and they can only be defended as plausible and systematic.

In order that bridge engineers may compare with their own practice, I have given several illustrations after the assumed formulae.

SAFE STRAINS FOR THE DIFFERENT MEMBERS OF A BRIDGE.

33. Weyrauch (see "Constructions of Iron and Steel," Chap. XIII, or this Magazine for June 1877, p. 519) deduces from Wohler's experiments, in connection with Launhardt's formula, the following value for the safe strain in kilograms per square centimeter=b, to which wrought iron should be subjected in tension,

$$b = \frac{2100}{n}(1+\tfrac{1}{2}\theta) \quad . \quad . \quad . \quad (9)$$

where n=factor of safety, $\theta = \frac{\text{min. B}}{\text{max. B}} =$ Minimum strain that piece ever bears / Maximum strain that piece ever bears

In the experiments, *impact*, such as a train of cars passing over a bridge causes, was not considered, hence its effect is not included in the above formula. There are no experiments to determine its effect, hence if we estimate it at all, it must be empirically. It is of course most convenient to embrace it in the formula in place of considering it separately.

34. Now it is reasonable to suppose that the effect of impact diminishes as the weight of the member increases. The weight of the web members of a bridge increases pretty regularly from the center to the abutments in a framed truss. θ increases from the center outwards, most rapidly at first, as an example will show. If now we assume the effect of impact to vary directly as θ, this hypothesis may be as near the truth as the first one. The unknown term now is the coefficient of θ, which call 1, and write for *the safe strain on ties in lb. per square inch.*

$$b = 7500\ (1+\theta) \quad . \quad . \quad . \quad (10)$$

Also write empirically for *the safe strain on wrought iron columns in lbs. per square inch*,

$$b = \frac{1}{4+\frac{1}{10}\left(\frac{l}{d}\right)} \cdot \frac{38500}{1+c\left(\frac{l}{r}\right)^2}(1+\theta) \quad . \quad (11)$$

where $c = \frac{1}{36000}$ for pillars *flat at both ends* and $c = \frac{2}{36000}$ for *both ends hinged*, and $c = \frac{1}{24000}$ for *one flat and one hinged end*. As before l=length of pillar, d =diameter in the direction of bending, r=radius of gyration of cross section about neutral axis; f has been taken here as a mean, 38500. It should be

determined by experiments for any particular form of cross section.

35. These formulæ are empirical, and can only be compared with the practice of leading engineers, there being no experiments to check them by. The experiments given by Stoney (Strains, &c., arts. 472, 475) rather give a color to, than disprove the formulæ. They certainly embrace the deduction of Launhardt that b varies as θ, and are supposed to include impact by diminishing b more than Weyrauch's formula towards the center of the truss.

The applications will now enable the engineer to see if the formulæ are any improvement over usual rules of thumb, and hence whether they may be adopted in whole or in part, or neglected altogether.

36. Assume that the chord strains are to be determined by supposing the load on bridge (say *two locomotives and tenders weighing* 2,800 *lbs. per foot, followed by cars weighing* 2,240 *lbs. per foot*), uniformly distributed and that the weights of bridges of 100, 200, 300, and 400 feet spans are 900, 1,500, 2,400 and 4,000 lbs. per. foot. We have, eq. (10),

For	lbs.
100 ft. span, $b=7500\ (1+\frac{900}{4100})=$	9,325
200 ft. span, $b=7500\ (1+\frac{1500}{4100})=$	10,300
300 ft. span, $b=7500\ (1+\frac{2400}{4100})=$	11,250
400 ft. span, $b=7500\ (1+\frac{4000}{4100})=$	12,190

For bridges of small weights $b=7500$ nearly. At the middle of all the bridges $\theta=0$ for ties and counter ties and $b=7500$; hence b varies from 7500 at center of bridges to 12190 for chords for a 400 feet span, or to 10300 for a 200 ft. span, &c. The excessive vibration of very small spans may have to be allowed for separately.

In formula (11) for posts, for $\left(\frac{l}{d}\right)=10$ and cylindrical columns, we get very nearly the same values as above, which appears at least reasonable, if not proper.

37. In the computation of $\theta=\frac{\text{min. B}}{\text{max. B}}$ for the web members, using the method of apex loads, we observe that a web tie is strained *most* when the load (engines in front) extends from farthest abutment to foot of tie, *least* when it extends from nearest abutment to top of tie. Thus, in an ordinary Pratt Truss 200′ span, 16 panels and weighing 272 lbs., we find from a comparison of shearing forces from eq. (10)

1st panel from end, $b=7500\times1{,}337$ $=10{,}027$

2d panel from end, $b=7500\times1{,}327$ $=9{,}942$

3d panel from end, $b=7500\times1{,}304$ $=9{,}780$

4th panel from end, $b=7500\times1{,}261$ $=9{,}458$

5th panel from end, $b=7500\times1{,}186$ $=8{,}895$

6th panel from end, $b=7500\times1{,}060$ $=7{,}950$

7th and 8th panel from end, $b=7500$.

For the counters we, of course, have $b=7500$. Nearly the same figures apply to posts when $\frac{l}{d}=10$.

38. It will be observed that for 200′ span we have $b=10000$ lbs. about, for chords and end ties. The above values of b for end ties and middle ties will give about the same dimensions as the rule "add 50 per cent. to live load for counters and middle bars," which is in use by some engineers.

Observe that for small spans, 10 to 30 feet, say, as θ is small, $b=7500$ nearly for all bridge members. (It may be best to take it less for such small spans). By the 50 per cent. rule, whilst the chord and end members are supposed loaded with the actual load, the middle ties are supposed loaded to nearly 50 per cent. over actual load. That b is nearly constant for small spans seems the more reasonable conclusion.

In the Louisville bridge 400 feet span, b for tension, was varied from 7000 to 12500 nearly, as by form (10).

In Europe, Gerber *used* nearly the same values as we derive from eq., (10), in his "Mainz Bridge" (see Weyrauch, Chap., XXX.)

If we write $b=646(1+\theta)$ kils. per sq. cent., the results agree very closely with Gerber's last formulae except for $\theta=\frac{3}{4}$ to 1 say.

In formula (10) above when $\theta=1$, we suppose impact null, $b=15000$ lbs., per square inch. It seems that this is sufficiently great—by Gerber's formulae it would be much greater.

39. Formula (10) then agrees somewhat with the practice of engineers, though b should diminish in each panel towards the center according to Wohler's law, which is established beyond question, and this is often neglected. It seems that the same reasoning should apply to posts.

40. Let us now compare eq., (11) with the table art. Thus for end braces of a 200 feet span, $\theta=\frac{1}{3}$, for hollow cylindrical posts $r=\frac{d}{4}\sqrt{2}$, we have,

$$b=\frac{1}{4+\frac{1}{10}\frac{l}{d}}\ \frac{385000\times 1\frac{1}{3}}{1+\frac{2}{9000}\frac{l}{d^2}}.$$

$$\because \frac{l}{d}=10,\ b=10{,}050$$

$$20,\ b=7860$$

$$30,\ b=6110$$

$$40,\ b=4730$$

$$50,\ b=3666$$

These figures agree very well with those recommended by the Committee of the Am. Soc. Engs. Formula (11) now causes h to diminish towards the center of the span, which *should be*, according to Wohler's law.

The above formulae are of course to be amended from time to time as experience may suggest. It is hardly probable that experiments can ever estimate the exact influence of impact as it occurs on bridges of all spans.

May we not as well estimate for it empirically now as to wait years to begin our guessing?

41. If it is preferred we may use larger values than 7500 in eq., (10) or 38500 in eq., (11), thus giving greater values for b than 1000 pounds per square inch for the chords of a 200 feet span.

It is the decrease of b for web members towards the center that should be the object of formulae, and that is effected by eqs., (10) and (11) in a simple manner. It is true as has been suggested by a friend that this is building faster than we have foundations laid, but as the bridge builder must estimate b by some rule of thumb, may not the above rules be defended on the plea of necessity, as well as other rules?

It must be distinctly understood that this is all that is claimed.

If a constant factor of safety is used in eq., (11), [the variable factor, $\left\{\frac{1}{4+\frac{1}{10}\frac{l}{d}}\right\}$ is only used out of abundant caution], it becomes simpler. This is a matter for practical bridge builders to decide.

COMPARISON OF TWO BRIDGE TRUSSES.

42. Two trusses are represented in Figs. 7 and 8 (the latter being a design of my own, though I am informed that a similar truss, only with vertical interior posts, has been used on the Pa. R.R.) The dotted lines represent suspenders to hold up the roadway.

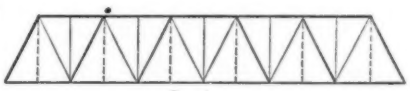


Fig. 7.

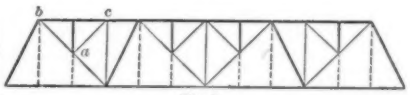


Fig. 8

The Fink elements *abc* (Fig. 8), support the upper chord, thus giving the same chord lengths as in Fig. 7. Weight of bridge was assumed at 272,000 lbs., span 200 feet, 12 panels of $16\frac{2}{3}$ feet each, height, Fig. 7=28 feet. Live load for *web* 2,800 lbs. per. foot on 100 feet, followed by 2,240 lbs. per foot for rest of span. For chords (although this does not give maximum strains), uniform live load of $(280000+224000)\div 200$ per foot. The load $\frac{1}{2}$ panel either side of an apex, supposed concentrated at that apex.

43. We have then for chords, $\left(\frac{l}{d}\right)=10$, since $\theta=\frac{272}{776}=.35$, $b=7500\ (1+.35)=10130$ for tension and nearly the same for compression.

From the shearing forces (which are a maximum when load extends from farthest abutment, a minimum for the same) that can be found by formula, or by tabulating for separate weights, we get,

Panel.	Shearing Forces.		θ
	Max. B.	Min. B.	
1	184,700	62,330	.333
2	153,960	49,040	.320
3	124,750	33,800	.272
4	97,100	16,600	.17
5	71,000	— 2,540	0
6	46,450	—23,650	0

From which we find b from eqs. (10) and (11) for ties and braces, b thus varies for ties from 7500, suspenders and middle ties, to about 10000 for end ties and chords. For Fink tie $\theta=\frac{9}{43}$ about $\therefore b=9100$. For vertical post θ was taken $=0$. The least section of post taken three square inch for Fink post and 4.5 square inch for vertical post of Fig. 7. The end upper chord panels regarded as "fixed at one end and hinged at the other," the other panel lengths as fixed at both ends; the chord and posts of wrought iron hollow cylindrical; diameter assumed for upper chord and end braces, $13\frac{1}{8}$ inches. We thus find the weights in lbs. (on the computed strains only) on $\frac{1}{2}$ of one truss.

	Fig. 7.	Fig. 8.
Upper Chord.....	8,200	9,500
Braces and Posts...	14,000	10,500
Lower Chord.....	7,700	6,800
Ties.....	5,800	7,300
	35,700	34,100

44. Thus giving a saving of 4×1600 lbs. of wrought iron on 2 trusses. If the same thickness of metal is taken for upper chord and braces (not considering the extra cost of mouldings, pins, &c.) the saving is found to be 13,000 lbs. The true saving lies then between 6400 and 13000 lbs.

If the engines and cars are distributed so as to give the maximum strain on chords, the saving is slightly less.

45. If we take the height of the truss, Fig. 8, as 31 feet, the diameters of compression members being the same as for Fig. 7, we find the saving on 2 trusses, 8,000 lbs.

46. In this Magazine for January, 1877, is an article by Emil Adler, C. E., in which a very general discussion of the "Most Economical Depth of Straight Girders and Trusses" is given.

Extending his method by including form (11) for posts, various inclinations of ties and braces, &c. I find from the formula deduced, about the most economical depth for the simple triangular of 12 panels (Fig. 7) to be 28 feet, for the truss, Fig. 8, about 31 feet. For a greater number of panels the depth is less.

The simple triangular, being one of our cheapest forms—theoretical—the saving in stress, Fig. 8, may be of interest to bridge engineers. The interior posts of Fig. 8 may be vertical, as square joints possess a decided mechanical advantage over hinged joints, being besides more economical.

47. In Fig. 8 the long tie at middle, that becomes at times a counter, may be rested on the bottom of the Fink post, at its center, thus saving in the weight of this latticed brace.

48. The different inclinations of ties and braces was suggested by the following demonstration:

Let P=shearing force on tie BC and brace AC, the panel length being AB=l. Put BC=l_2.

AC=l_1, AD=x, DC=h.

W=weight of 1 cu. in. of tie in lbs.

W′=weight of 1 cu. in. of post in lbs.

C=cost per lb. of tie in cents.

C′=cost per lb. of post in cents.

T=safe strain per sq. in. for tie.

S=safe strain per sq. in. for post.

We have

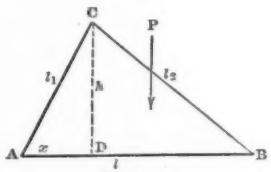


Fig. 9

Strain on CB=$P\frac{l_2}{h}$; Cost CB=$\frac{Pl_2}{hT}12l_2wc$.

Strain on CA=$P\frac{l_1}{h}$; Cost CA=$\frac{Pl_1}{hS}12l_1w'c'$

Total cost of tie CB and post CA=B

$$\gamma=\frac{12P}{h}\left(\frac{l_2^2}{T}wc+\frac{l_1^2}{S}w'c'\right)$$

$$=\frac{12P}{h}\left(\frac{h^2+(l-x)^2}{T}wc+\frac{h^2+x^2}{S}w'c'\right)$$

Now if we desire this to be a minimum, T and S remaining constant and x varying, we put its first differential coefficient $=0$.

$$\therefore \frac{-2lwc}{T}+\frac{2xwc}{T}+\frac{2xw'c'}{S}=0.$$

$$\therefore x=\frac{Swcl}{Swc+Tw'c'}$$

Thus, for $wc=w'c'$,

$$T=11000,\ S=5000,\ x=.31,$$
$$S=6000,\ x=.35,$$
$$\&c.,\ \&c.$$

We see that for $wc=w'c'$ as long as S and T are different, the ties and braces should have different inclinations, and for proportions above $x=\frac{1}{3}$ about.

49. In a Pratt Truss of 200 span if ties and braces are so inclined in them that $x=\frac{1}{3}$ there is a saving in them alone of a few thousand pounds. The weight of the counters is of course increased, owing to their increased length.

The economy of the square joint may, however eliminate all saving. If we suppose the post AC of wood, we shall find x to be very nearly equal to l, hence in the Howe bridge the braces should be of wood. The Howe form is generally applied to wooden bridges. For the post AC of cast iron, $x=\frac{1}{2}$ about.

50. If a weight is supposed at top of post, its effect is to require that the post be more nearly vertical. Its influence is easily included in the formula.

51. The inclination of the Fink ties, $\overline{ab}$, $\overline{ac}$ of Fig. 8, has been taken the same as that of the main ties. This is best for appearance sake.

To find the inclination that secures the greatest economy in the ties, post and chord of a Fink element (neglecting the bracing between the two trusses of a bridge) we may proceed as follows:

Let T=safe strain on ties AC, BC Fig. 10, and S and S′ the safe strains per square inch for the post $\overline{PC}$ and the chord AB, P being the load, $\overline{PB}=l$, BC P$=\theta$. The strain on BC or AC$=\frac{1}{2}$P sec. θ.

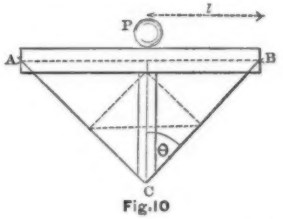


Fig. 10

$$\therefore \text{Volume of AC}+\text{BC}=\frac{P\sec.\theta}{T}l\operatorname{cosec}\theta$$

$$\text{Volume of Post }\overline{PC}=\frac{P}{S}l\cot.\theta$$

$$\text{Strain on AB}=\tfrac{1}{2}P\tan.\theta$$

$$\therefore \text{Volume of AB}=\frac{P\,l\tan.\theta}{S'}$$

$$\text{Total volume}=Pl\left(\frac{\sec.\theta\operatorname{cosec}\theta}{T}+\frac{\cot.\theta}{S}+\frac{\tan.\theta}{S'}\right)$$

to be a minimum. Whence,

$$\frac{1}{T\cos.^2\theta}-\frac{1}{T\sin.^2\theta}\cdot-\frac{1}{S\sin.^2\theta}+\frac{1}{S'\cos.^2\theta}=o$$

$$\therefore \frac{S'+T}{TS'\cos.^2\theta}-\frac{S+T}{TS\sin.^2\theta}=o$$

$$\therefore \tan.\theta=\sqrt{\frac{(S+T)\,S'}{(S'+T)\,S}}$$

52. Some numerical values will best illustrate the changes of θ, for the greater economy. Thus let S′$=10000$ lbs.,$=10$ (thousand lbs. being understood).

$$\frac{T}{S}=\frac{10}{10},\quad \theta=45^\circ$$
$$\frac{T}{S}=\frac{10}{7},\quad \theta=47^\circ 47'$$
$$\frac{T}{S}=\frac{10}{6},\quad \theta=49^\circ 3'$$
$$\frac{T}{S}=\frac{10}{5},\quad \theta=50^\circ 47'$$
$$\frac{T}{S}=\frac{10}{4},\quad \theta=52^\circ 55$$

The formula may be made to include cost and specific gravities of ties and compression members if desired.

A considerable saving may be effected in the Fink bridge by using the above formula. For complete accuracy the transverse bracing should be considered

also. For slight changes of θ, it does not vary, though, as rapidly as the vol. of ties and columns.

53. With regard to some of the foregoing subjects the writer invites criticism, so it be just and its aim—the Truth, for in finding the last he is as much interested as any one; but an intemperate attack too often degenerates into personality, which "though it make the unskillful laugh, cannot but make the judicious grieve."

ANCIENT ROMAN WORKS.*

From "The Builder."

No relics of the period which preceded the Gothic invasion affords us such an insight into the rough yet civilized manners and customs of the citizens of Rome, and of the vast multitudes who bowed to the imperial rule, as do the stately amphitheatres whose ruins still grace so many Italian towns, though earthquakes have levelled some, and tempests are gradually, but not less surely, destroying those which yet remain.

It is proposed to arrange the following remarks in three divisions:—First, briefly dwelling on the origin and history of the Roman amphitheatres; secondly, on the plan and general arrangement; and thirdly, on the construction and materials employed in their erection.

It can hardly be doubted that the Romans obtained their ideas of amphitheatres from the Etruscans, and the remains at Sutri are a strong argument in favor of this belief, where we see a natural amphitheatre formed on the sloping sides of a hill, with the seats hewn out of the solid tufa rock. The Greek amphitheatres were also constructed in the same manner; sometimes Nature herself formed the amphitheatre almost without the labor of man, and at other times an ancient stone quarry was converted by small trouble into an amphitheatre accommodating many thousands of spectators. Such amphitheatres as these can scarcely be said to have been erected, since they were chiefly formed by excavation, and it remained for the Romans under C. Scribonius Curio to erect the first building of this kind, in Rome itself; though history gives such strange accounts of this structure that they sound almost fabulous. It is supposed to have consisted of two wooden theatres placed back to back and made to revolve on pivots, so that by means of windlasses and machinery, detailed by Pliny, the *visorium* of each could be brought round face to face and united on the line of its diameter, or it could be used as two separate theatres. Pliny narrates that it was formed "of two large theatres of wood mortised together in a singular manner, and suspended so as to turn freely, on both sides of which were exhibited the afternoon shows of plays."

The name "amphitheatrum" was first given to the building in the Campus Martius, erected soon after this by Julius Cæsar, and which was also of wood; but both these were probably destroyed by fire, and Statilius Taurus, in B. C. 29, erected the first stone amphitheatre at the command of Augustus. This was also situated in the Campus Martius, but the seats and the whole of the interior being of wood, it met the same fate as its predecessors. The Romans at this period had grown enamored of gladiatorial contests, the excitement of which became almost as necessary to them as food; and as the Imperial city was now left without any building of this class, Vespasian resolved to erect an amphitheatre which should eclipse all former ones, and the Colosseum, or the Flavian amphitheatre, was commenced. Situated between the Esquiline and the Cœlian hills, on the site of the great artificial lake which washed the walls of the Golden House of Nero, it stood almost centrally in ancient Rome. This enormous structure took only ten years and nine months in building, during which time 15,000 men are said to have been employed on it. Nevertheless, Ves-

* By Mr. W. Hilton Nash. Read before the Architectural Association.

pasian did not live to see it completed, and Titus dedicated it in A. D. 80, although it was not fully completed till the reign of Domitian. The history of this building is perhaps more varied than that of any other, it having been used during the eleventh and twelfth centuries as a castle by the Frangipani family, in 1450 for a hospital, and in the seventeenth century for the representation of the Passion plays so much in vogue during the Middle Ages, and finally in the year 1700 a saltpetre manufactory was carried on within it. Wherever the Romans came with their conquering legions, we find traces of their amphitheatres; so that not only in Italy, as at Rome, Verona, Polo, Pozzuoli, Pompeii, and Capua, but also in France, at Nismes and Arles, and in England, at Cirencester, Silchester, Dorchester, and Old Sarum; and in many other countries have these interesting remains been discovered. But when we reflect on the gross butchery which these ancient buildings have witnessed, when the seats were filled, tier upon tier, with thousands of spectators crying out in the common language of the amphitheatre, "Kill him, tear him, burn him! Why is he so fearful of death?" our thoughts are sobered, and we cease almost to regret that the buildings are in ruins.

Let us now consider the plan and arrangement of the Roman amphitheatre. There are three main features in all structures of this class:—the arena; the *gradus spectatorum*, or seats for the spectators, forming altogether what was termed the *visorium;* and the colonnades and vomitories, or staircases, by which access to the seats was obtained. The arena itself varies in size and proportion, but is usually about one-third of the shorter axis of the building; it was usually covered with sand, as its name denotes, but was sometimes boarded. A wall about fifteen feet high, termed the podium, surrounded the arena, for the purpose of protecting the spectators from the enraged lions or the bounding panther; and on this wall were rollers which turned in sockets, so that the animals could not climb it, the summit being frequently protected by net work, as in the Colosseum, where Nero, in a fit of extravagance, is said to have fastened the nets with amber-beads. In addition to this, ditches or *euripi* sometimes surrounded the arena as a further safeguard against any accident. On the summit of the podium in the Colosseum were the seats of distinction for the senators and the emperor himself; above this were the seats for the equestrian order, or, as we should say, the upper classes; above these, divided by a *præcinctio*, or landing, were the seats for the *popularia*, or middle classes; and the upper seats of all were occupied by the *pullati*, or lower orders. In the Colosseum a colonnade runs round above this, not usually met with in other buildings, which was appropriated to the women, and on the flat roof of this colonnade were stationed the sailors who were engaged in the management of the velarium. Each story was divided from the other by a præcinctio, and intersected at intervals by the passages leading to the various staircases. The oblong form of all amphitheatres, except the double theatre of Curio and Cæsar's wooden theatre, which were circular, is a feature which we may do well to consider. These buildings were frequently used, not only for combats between man and beast, but between small bodies of armed men, and by the elliptical form of the arena these gladiators were better able to extend their line, and appeared to more advantage than if they had been placed in the centre of a circular platform with little scope for diversity of movements. The Pompeian frescoes afford illustrations of this, for there we seldom see the combatants massed, but in a long extended line. The scorching rays of an Italian sun made it necessary to have some covering over the amphitheatre during the games, since the people sometimes sat for six or eight hours together, and often took their places over night to secure a good seat; on this account a large awning called the *velarium*, made of woollen cloth or silk, extended over the whole of the visorium. The difficulty of raising and maintaining in its position this vast awning must have been great, when we remember that the least diameter of the Colosseum is nearly equal to the length of the Menai Bridge, and, in fact, in boisterous weather, the awning could not be hoisted, and when this was the case the Romans came to the show in broad hats and with a kind of parasol called

umbella. Fontana, in his interesting work, gives a curious illustration of the velarium, but no description. Gwilt, however, supposes it to have been arranged in the following manner:—A cable being placed round the edge, and following the curve of the podium, strong ropes were attached to it in the direction of the radiating walls on the plan, and passing through pulleys in the heads of the masts ranged round the building and resting on corbels; there were 240 of these masts in the Colosseum, each one passing through a hole in the cornice and socketing into the corbels below; these ropes were then brought through the heads of the masts and down to the ground, where numerous windlasses were fixed to receive them, and at a given signal every windlass commenced its work, and the immense tent moved slowly upwards till it arrived at the level of the upper cornice, and when fixed in its position, sloped gracefully towards the centre; the central part, viz., that over the arena, being left open to the sky. The cloth itself might have been moved on rings attached to the main ropes, and worked by persons stationed on the parapet. The hoisting of the velarium was in Rome always entrusted to sailors, many hundreds being employed, and encamping during the gladiatorial shows in the neighborhood.

We now come to the third division of the subject, viz., the construction and materials employed. The construction of almost all amphitheatres was a succession of cross walls converging towards the centres of the different arcs, intersected by other walls of gradually increasing diameter, pierced by arches on which were bedded the seats for the spectators, and four principal gateways were placed at the extremities of the major and minor axes of the ellipse. The Colosseum at Rome, though not so skilfully or carefully built as some other examples, is, as it were, an epitome of all other amphitheatres. This vast structure had four corridors running entirely round it on the ground level, the two largest being on the exterior, and forming a sort of covered portico or ambulatory, each corridor being roofed by a continuous semicircular vault supported on square piers. Between the second and third corridors is a large space occupied by staircases leading to the various seats; the entrances to the staircases being from the second and third corridors; from the latter also are other entrances into staircases which are in another space between the third and fourth corridors. The third corridor, which must have been very dark, received a small amount of light by holes in the vault. We are apt to affirm that the ancients never built hurriedly, but in this vast structure, which some maintain only took ten years and nine months to build, we see striking evidences both in the work of Vespasian and Titus of the rapid way in which the works were pushed on. Carelessly-dressed stones—many of the arcades being unequally spaced out—no mouldings preserving the same level throughout the building, and the profiles of the mouldings themselves in some cases unfinished, all tend to strengthen this belief; and yet, in spite of all this loose detail, there is perhaps no building in the world which has called forth such universal admiration as the Colosseum. The general effect of the Colosseum would have been far finer without the upper or fourth story, which is so heavy and unmeaning that some have even doubted whether it was part of the original structure, and I believe this is the only amphitheatre which has this addition. The amphitheatre at Verona has only three stories, and the exterior elevation has consequently a more æsthetic proportion. Mr. Ferguson conjectures, in his "History of Architecture," that this upper story may have been added for the purpose of working the velarium, and this seems more probable, since to cover so vast a surface with an awning would be next to impossible without some space on which the sailors who worked the velarium could carry on their operations; and this would be provided by the flat roofing covering the colonnade. Mr. Parker is of opinion that this story is of the third century, and that there was originally one of wood of a somewhat similar design. The system of Roman vaulting has been so ably described by Mr. Phené Spiers in his paper on the "Roman Baths" that I can only briefly dwell upon it; for, indeed, the vaulting which exists in the ruins of amphitheatres is of a most elementary character, its purpose being usually to

roof the passages leading to the seats, and for supporting the sub-structures. The principal materials employed in the Colosseum are stone, brick, concrete, and pumice stone. Tiburtine stone, or, as it is now called, Travertine (though the former is the more correct name, as the original quarries were on the banks of the Tiber, near Tivoli,) forms the facing of the outer walls, also the piers of the two outer corridors, the centres of the arches, and some bands in the inner walls. The stone is largely used at the present day in Rome, and forms an excellent facing stone, almost like marble. The stonework in many cases is not bonded with the brickwork, and in some instances has entirely separated from it, leaving a straight vertical joint. Large blocks of tufa are also used for the filling in, but the Roman architects did not consider the tufa walls, or even the tufa and brick combined, capable of sustaining the compression which the numerous rows of seats and the vast concourse of spectators who occupied them would necessarily subject them to: so at frequent intervals they cut away the brickwork and inserted piers of travertine to assist in carrying the weight. The stonework of the Colosseum was originally held together by iron cramps; these have rusted, and eventually split the stone, and have fallen out or been carried away, perhaps to be used again to cause more similar destruction. I know of no example of the *opus reticulatum* in the Colosseum; nevertheless, it was commonly used in contemporary buildings. It consisted of small pyramidal blocks of tufa, flat on one side, and about $2\frac{1}{2}$ inches by $2\frac{3}{4}$ inches square on the face, and pressed into a bed of concrete while wet, and forming, as its name implies, an appearance like network. The main body of the work is of brick, and some of the finest quality. The bricks are tile-shaped, about one foot by one foot six inches, and were called by the Romans *lateras*, and formed, when bedded, the famous *opus lateritium*. The first century produced the finest brickwork the world has ever seen, and in Nero's time we have ten courses to the foot of these tile-like bricks, laid with an almost imperceptible mortar joint; but after the reign of Trajan, the bricks were made thicker and the mortar-joints also increased, being often one inch thick, till, in the fourth century, and during the reign of Constantine, we find the brickwork measuring four courses to the foot, and by knowledge of these facts it is easy to ascertain the date of any Roman brickwork with tolerable accuracy. The Colosseum possesses good specimens of first-century brickwork, and also the Amphitheatrum Castrense in Rome, and also some contemporary brickwork from the Palace of Nero, date A.D. 60. Here we see the tile-work called *opus lateritium* bedded in mortar composed of puzzolana and lime in proportions of about three of the former to one of the latter.

The earliest instance of the use of concrete (*fartura*) at Rome is at the ruins of the Emporium, B.C. 195. A vast amount of concrete was used in the Colosseum, chiefly for filling in the vaulting and internal walls; but finding this material too heavy, pumice-stone was employed for the vaults, as it could easily be obtained from the neighborhood, and is found at the present day mixed with the Roman tufa in beds a few inches thick. All concrete is liable to fracture by the shrinkage of the material, and in walls of any great length the fractures occur at equal distances apart (as in the Roman walls of Richborough Castle). Whether the Romans were aware of this fact or not is uncertain, but they frequently introduced layers of thin bricks at intervals to tie the whole mass together, and perhaps more with the idea of bonding the work than of counteracting its shrinkage. This tile-bonding may be seen in almost all Roman concrete walls in Europe, and in the Temple of Minerva Medica at Rome.

Though not strictly within the province of an architectural paper, it may be well to touch briefly on the sports and combats which took place on the arena. The gladiatorial encounters were either between themselves or with wild beasts, and special schools were instituted where the gladiators underwent rigorous training. Gladiators were first exhibited in Rome in B.C. 488, by M. and D. Brutus; and they were all bound by a solemn oath—"We swear, after the dictation of Eumolpus, to suffer death by fire, bonds, stripes, and the sword, and whatever else Eumolpus may command, as true gladiators we bind ourselves body and soul to

our master's service."—(Petronius.) In the early days of Rome, before the public taste became degraded, the office of gladiator was confined to the lowest classes; but the thirst for popular applause induced men of good family, and even senators, to enter the lists; and hence we find an edict forbidding persons of exalted rank from entering the arena. Previously to the performance of any spectacle the walls were placarded, and notices (*libelli*) issued by the editor or showman, stating the nature of the exhibition and the names of the combatants, and these notices were frequently illustrated, after the manner of our nineteenth-century play-bills. On the appointed day, those who had not previously obtained tickets took up their position at daybreak, and sometimes over night. The *equites*, the *popularia* and *pullati* being seated, were followed by the senators, who had previously sent their *biselli*, or chairs of state, to be placed on the podium. Then a flourish of trumpets and a grand procession announced the arrival of the emperor, who, sitting at the *suggestus*, or royal box, gave the order for the sports to commence. At first the combats are comparatively tame, but as soon as the fiery blood of the gladiators is roused, they throw away the wooden staves with which they have been fighting, and take to the sword and buckler, and the *retiarius*, with his net and trident, seeks to entangle and despatch the *secutor*, who follows him round and round the arena with his deadly knife. The victor looks up at the vast sea of heads, and awaits the final signal of the up or down turned thumbs of the spectators before taking up his sword to dispatch his victim.

The death-signal is given, the sword descends, and the corpse is dragged off the arena by a large hook to the *spoliarium*, through the Porta Libitinensis, or Gate of Death. And now the great trap-doors on the surface of the arena are seen to open, and vast wooden cages appear, containing the wild beasts, which "leap out of the earth," as an old writer has it. The cages open spontaneously, then sink into the earth again, and the ferocious animals let loose on the arena are slaughtered by the bloodthirsty gladiators. The apparatus for bringing the wild beasts on to the arena in this manner only existed in some amphitheatres, but not at Pompeii or Verona. The recent excavations at the Colosseum have brought to light traces of the grooves in which these lifts or *pegmata* worked, and also the chase for the counterpoises, and the sockets in which turned the pivots of the capstans used for working the apparatus. These lifts may also have been used for bringing up the ships which were to take part in the *naumachia* or naval engagement, and in the Colosseum a vast gulf separated the arena into two parts, so that when the boards which covered it were removed any large stage properties could be lifted on to the arena. These sub-arena buildings or sub-structures were, perhaps, the most interesting portion of the amphitheatres, and, as it is the ambition of many persons to see behind the scenes of a modern theatre, so it must have been the desire of many a Roman citizen to see below the arena and get an insight into the mysteries which caused wild beasts to spring out of the earth and ships to float on the surface of the arena; for Seneca writes,—"The eyes of the silly people are astonished at all these sudden movements, the causes of which they do not understand." This species of amusement gave its name to the buildings which were specially devoted to them, and those at Rome were very similar to excavated amphitheatres. The *naumachia* of Augustus was 1,800 feet long by 1,200 feet broad; and at the sea-fight held there during the reign of Julius Cæsar 4,000 seamen and 1,000 marines were engaged. These *naumachia* were constructed of stone, as we read that that of Domitian was pulled down to repair the Circus Maximus. No amphitheatre was capable of being used as a *naumachia* unless provided with vast underground works and drainage on a large scale; the amphitheatres of Verona and Pompeii, for this reason, could never have been used for this purpose, but in those of Rome, Capua, Pozzuoli, and Pola, these entertainments were undoubtedly provided. Representations of the chase were not wanting among the amusements of the arena, and trees torn up by the roots were transplanted on its surface, and wild boar and stag hunts took place in these temporary forests, as may be seen depicted in the

frescoes at Pompeii; and Calpurius describes the visit of a country lad to Rome, who relates all the wonderful things he saw there. "We saw the amphitheatre, with interwoven beams rising to heaven. . . . . I saw all kinds of wild beasts, ... not only those carnivorous monsters of the forests, but sea monsters, together with fighting bears." Sixty-two amphitheatres are enumerated by Clerisseau in his "Antiquities of France," as still existing; but it will hardly be necessary to mention all of these, as the remains of some are scarcely traceable; however, we may consider a few of the more important ones. The great amphitheatre of Capua, which rivalled the Colosseum in size, is said to have held 87,000 spectators, but only two of the seventy-four arcades that formed the lower story exist. It had three stories, of the Doric order, and more pure in detail than the Colosseum. The sub-arena structures are more perfect, although the general arrangement of the plan is nearly identical. We see the same dens for the wild beasts, the sockets and grooves for working the *pegmata* or lifts by which they were hoisted on to the arena; but the arena, instead of being a boarded floor, is carried on vaulting, and pierced by openings for the lifts, and probably each of these trap-doors had a water-tight covering, to enable the whole of the arena to be flooded. If such was the intention, the surface would not be covered with more than 2 feet or 3 feet of water; while the boats or galleys for the naval engagements would float in the deep canals, three in number, which run parallel with the major axis of the ellipse, the other water being merely for creating an impression on the spectators that the whole surface was flooded to an equal depth. The *opus reticulatum*, surrounded with a framework of brick, which is found in this structure, proves it to have been partly built in the time of the Emperor Hadrian, and it is probable that a greater part was executed in the second century.

It stands on the site of the ancient city of Capua, now called Santa Maria di Capua Vetere, and three miles beyond the modern town of that name. It is not positively known by whom the amphitheater of Verona was erected, yet some writers attribute it to the time of Augustus, and the good preservation of the interior is owing to its having been used for plays during the middle ages; and this practice is still continued, for when I visited this picturesque city in 1875, a stage was erected in the amphitheater, and a play was acted one Sunday afternoon. Of the outer wall which supported the portico surrounding the building there remain only seven piers. At the two extremes are the principal entrances, the sides of which are parallel. I think the entrances at each end give great grandeur to the design, as they are more marked and architecturally treated than in most examples. On the interior over the two entrances are galleries inclosed on their front and sides by a balustrade, and the seat of distinction was situated here. It is later in date, says Fergusson, than the Colosseum, Capua, or Nismes, on account of its rustication; this, I think, is doubtful, especially as this rustication is said to have been unintentional, and simply caused by their not being time to complete the work as intended. The idea of rusticating piers and columns is said to have originated from this building. The amphitheater at Pola, in Istria, is about the same age as that at Verona, but the arena is almost entirely destroyed, and only the outer walls remain. The interior may possibly have been of wood, and has either decayed or been burned; the dimensions are nearly the same as Nismes, viz., 436 feet by 346 feet, and 97 feet high. The two lower stories have pilasters, but the upper one has none, and was probably built so as to allow of the masts which carried the ropes of the velarium to come down and socket into the cornice of the second story; and there is an open kind of battlement terminating the attic story, which it is conjectured had some connection with the working of the awning. There are here the usual canals for conducting water for the purposes of the *naumachia*, lined with water cement, but the substructures only go under part of the fabric, since the labor of excavating under the whole surface would have been enormous, as the building is situated on the slope of a rocky hill. Four rectangular towers are attached, which were probably provided with wooden stairs, as no remains

of stone ones are discernible, but the use of these towers has puzzled the most learned antiquaries, and no satisfactory solution of the problem has yet been arrived at. The Amphitheater of Puteoli or Pozzuoli, near Naples, has substructures as fine as any known amphitheater, though in size it is inferior to the Colosseum or the Capuan amphitheater. It is probable that it was used as a *naumachia*, since we find a large central channel deep enough to float a Roman galley, and constructed after the same manner as that at Capua, with small square holes all round the arena, which have evidently been fitted with water-tight coverings to prevent the escape of water when the whole arena was flooded; this would have been quite practicable here, but in the Colosseum it could not have been done without some modifications of its existing plan. This building was chiefly erected in the time of Hadrian, and exhibits beautiful specimens of the construction of his time. The amphitheater of Pompeii accommodated about 10,000 or 11,000 spectators, and was constructed chiefly of rough masonry, called *opus incertum*, with quoins of squared stone, partly filled in with rubble, the whole originally having been covered with ashlar. The entrances are at each end of the ellipse. The podium was found to be richly ornamented with frescoes when first excavated, but this rapidly peeled off on exposure to the atmosphere. The amphitheater at Ostricoli is small, measuring only 312 feet on its major, and 230 feet on its minor axis, and is two stories in height. It is in a very ruined condition, though one of the most modern works of this class; nevertheless its simplicity of design makes it a valuable specimen of Roman art.

In France there are two amphitheatres of considerable size, one at Nismes in Aquitaine and the other at Arles, the former capable of containing about 17,000 and the latter 20,000 spectators. At Nismes, a wooden floor with trap-doors in it still exists, though Mr. Parker is of opinion that this is not the ancient floor, but replaced one of the same character. The substructures, which differ from those of the large Italian amphitheatres, appear to have had no stone staircase for access. At Arles there were originally some subterranean arches, two stories of sixty arches each and an attic, well constructed of carefully-fitted stones of large size, the orders employed on the exterior being the Doric and Corinthian. The artistic effect of these buildings is mainly due to their mass, combined with an elegance of curve which is always found in buildings of an elliptical form; the circle does not give the same variety or gracefulness of curve which is found in an ellipse. Some buildings strike us by their beauty of detail, but it is evident that this is not the first impression produced by the Colosseum or any other amphitheatre which astonishes us by its vastness. The solid tufa walls of the sub-structures, the numerous tiers of vaults upon vaults rising with their rugged outline against the blue background of the clear Italian sky, inspire us with a sense of reverence towards the builders under the Imperial rule, conquerors of the world, and masters of the arts and sciences, which we, even in these latter ages, seem hardly to have equaled. It does one good to contemplate a block of tufa, such as we see in the sub-structures of the Colosseum, discarding the bondage of mortar and sufficiently secure by reason of its enormous size, and bedded with that care which the early builders knew so well the value of. These solid structures make our mean brick walls, which are measured by inches, look like a sheet of paper compared with an armor-plate.

Some modern structures have been erected which in many points are analogous to the Roman amphitheatres, but a more difficult problem than that which the Roman architects had to solve has been successfully mastered by our modern constructors. In the Roman amphitheatres, all that was required was that every one should be able to see the doings in the arena, but we have to provide buildings in which an assembly can see and hear also; it is not sufficient for them to be spectators, they must be an audience. This difficulty has been met in a fairly satisfactory manner in the Albert Hall. No wild beasts are let loose on this arena, so no podium is required, and no English tars fight on its surface in ships as in the ancient *naumachia*, therefore no substructures or great drainage works are necessary; but there are features in this building

which carry out the Roman principles, though perhaps the staircases, owing to modern ideas, are not planned on the same simple and masterly manner; nevertheless, I should advise any one who wishes to obtain a faint idea of the grandeur of a Roman amphitheatre, and has not an opportunity of seeing the works themselves, to study this building. The shell of the Albert Hall or the "Kensington Amphitheatre" consists of two concentric walls, between which are contained the staircases, corridors, and general arrangements for the service of the central hall, and all the necessary offices, etc. The arrangement of the *cunei*, or wedges of seats, is amphitheatrical, and they are entered from spacious corridors and dormitories; but the simple and stately form of the inverted cone is broken by three tiers of boxes running round the building.

In considering all Roman works, it is well for us to understand as much as possible the sentiments which actuated the Romans in the construction of their edifices. Wealthy and proud, they delighted in showing their wealth in costliness of materials rather than in delicacy of form and thought—to strike wonder into strangers' minds, not by their refinement of thought as expressed in their buildings, but by massiveness and apparent and frequently real solidity which has seldom been surpassed. The power of mass is the first thing which strikes an ordinary beholder, and the Romans, as has justly been observed, were an arch-building but not an architectural nation. The arch and the vault, grafted on the Greek models, soon subsidised the vertical and horizontal lines which were the ruling features of Greek design. The Corinthian order supplanted the manly Doric, and I believe the decline of Rome may be dated from the time when the simple Doric and the graceful Ionic were almost entirely discarded, and the meretricious Corinthian became the universal order in all buildings. This is noted in M. Viollet-le-Duc's "Lectures on Architecture," so admirably translated by Mr. Bucknall, where we read, in reference to the degeneracy of art and the looseness of detail in Roman times in comparison with the Greek epoch, that Roman architects "had not leisure to study purity of line, the stone-dressers had not the time for these refinements; it is less trouble to describe a quarter circle with a compass than to find an indescribable curve," such as the Greek torus. In one important item the Romans far excelled the Greeks; they had the faculty of planning, and for my own part I never came away from studying a Roman plan, whether it be for a temple, a palace, a public bath, or an amphitheatre, without feeling braced and refreshed in mind, and strongly impressed with the skill of the Roman architects in this respect. We in the present day, and especially in this great capital, are in a position very similar to that of the Roman citizens, surrounded by wealth and comfort which borders on luxury; we live in an age as great in its way for building, if not for architectural work, as any era of ancient Rome; and we architects in this busy nineteenth century are pushed and hurried on in our work like the Roman architects of the first century; we cannot afford to give a whole day to the finessing of a curve or the rounding of a torus. Art is too long, and life is too short; or, we might put it, the work is too great, and the percentage too small, for architects to work in this fashion; and an architect should not be obliged to look out anxiously for another building to occupy his attention as soon as one is completed, but should be able to give his heart and soul to his work, and be free from all anxiety, which is incompatible with that tranquillity of mind from which emanate all noble designs.

In conclusion, I must express my regret that the scope of this paper has not allowed me to treat of the great drainage works which supplied these vast structures with water, and afterwards conducted it to the Tiber; for the aqueducts which conveyed the water from the distant hills over the dreary tracks of the Campagna, were among the clearest evidences of the grandeur of the empire. It was in this class of work, where the mechanical powers of mind and matter were brought into operation, that the Romans were *facile princeps*. The great drain of the Colosseum itself was a work of no small magnitude, and the small drains which ramify the soil of Rome like the veins and arteries of the human body, make it impossible to dig down more than fifteen feet without coming

upon some supply of running water. And now, as we draw near to the close, not only of these remarks but also of our session, I hope that some few of our members may become imbued with that enthusiasm which should accompany the contemplation of these noble works; for architecture must be studied with love or not at all; and when we separate, some to study the beauty of Gothic detail and revel in the grandeur of the grey and solemn minster, and others to seek the shores of sunny Italy, there to ponder on the past glories of the Eternal City, where, as the poet says:

> "The very dust we tread stirs as with life,
> And not a breath but from the ground sends up
> Something of human grandeur;"

let us look reverently on these structures, which from various causes have happily escaped the cruel hand of the restorer, and still go crumbling on in untouched solemnity, so that, as we traverse the arcaded Gothic cloister, or the deep, shady, vaulted corridors of the Colosseum, which we admire "as we admire the beautiful in death," our minds may haply be awakened to the spirit which actuated these early builders, who produced works which, as time rolls on, accumulate increased interest and veneration.

REPORTS OF ENGINEERING SOCIETIES.

Liverpool Engineering Society.—This society held its first meeting of the coming session in the rooms of the Royal Institution. Mr. Wilkinson Squire brought before the members some interesting samples of mortar. A piece from Sandown Castle, Kent, built by Henry VIII., was tested by Mr. Squire, proved to stand a tensile strain of 263 lbs. per square inch, the section tested being 1.8 square inches in area. A sample of mortar lately brought by Mr. Alfred Holt, member, from Nicopolis, a city built B.C. 40, to commemorate the battle of Actium, appeared to be as hard as stone. The President thought that, on account of its rough and honeycombed appearance it bore out his theory that the goodness of ancient mortar was due to the quality of the materials rather than to any particular care then bestowed upon its mixing. Mr. R. F. Pitt, member, read a valuable paper, entitled "Piecework *versus* Daywork." He endeavored to show that piecework is advantageous under most circumstances, the principals obtaining more work in a given time, and turning over their capital more quickly, at the same time the men make higher wages than they otherwise could do. He mentioned several instances where men earned by piecework double the current rate of wages, whilst men working beside them under similar conditions could hardly earn an ordinary day's wages. These men, by "daywork," or club rules, would have earned exactly the same; such methods of payment, therefore, tend to lower the skill of the artisan besides increasing the cost of the work.

Society of Engineers.—A party of about thirty of the members, associates, and friends of this society, on August 15th, paid a visit to the iron shipbuilding yard of Messrs. Samuda Brothers, at Poplar, and there inspected several first-class vessels now in course of construction. The party, headed by Mr. Clement Barnard, vice-president, and Mr. Perry F. Nursey, secretary, were received at the yard by Mr. J. T. Field, one of the managers of the establishment, and by him conducted to the objects of our visit. Proceeding through the worksheds, now in moderately active operation, the engineers were shown four new life-boats, two of which are screw launches, and a third, a row-boat, and were then taken to the hydraulic press, one of the largest in the world, and capable, when used in bending armor plates, of giving a pressure of 4,000 tons. Continuing the journey over piles of heavy timber and scraps of iron, the party arrived at the *Burgurafer*, an armor-plated ship with twin screws, now in course of construction for the Turkish Government. A sister ship, the *Payki Sherriff*, not quite finished, also for the service of the Ottoman Porte, was visited. She is 245 feet in length, 3,800 horse-power, with 4,700 tons displacement, and carries four 25-ton guns. The battery is within a strongly-armored turret, where enough space is obtained and gear laid down to vary the direction of the fire very considerably. Each ship carries ten inches of armor-plating. The unfortunate *Independencia*, whose back was broken in an unsuccessful launch at Messrs. Dudgeon's, some time ago, lies alongside, and is being rapidly repaired for the Brazilian Government. A beautiful ship the *Foo So*, designed by Mr. E. J. Reed for the Japanese navy, next received the attention of the engineers. She is 200 feet in length, of 3,500 horse-power, with 6,700 tons displacement, and is remarkable for spacious deck room and ample space for working her guns. She carries four 18-ton and two 10-ton steel guns, and is protected by armor-plating of 9 inches thickness. The *Foo So* is built in thirty-two separate compartments, in order to localize the effect of torpedoes, and such is her engine-power that a speed of thirteen knots an hour is expected to be obtained. The Turkish ironclads are detained in the Thames pending the present war.

IRON AND STEEL NOTES.

A singular fact with reference to the production of heat is described by M. Oliver (*Comptes Rendus*). A square bar of steel 15 mm. in width and 70 to 80 ctm. long is seized with the two hands, placed, one at one end, the other in the middle of the bar. The other end is pressed against an emery grind-stone rotating rapidly. In a few minutes the rubbed end is considerably heated. The band at the

middle has no sensation of heat, but that at the extremity is strongly heated, so that it has to be taken from the bar. Thus, in certain cases, heat appears not to be propagated in metals from one part to that next it.

FUEL USED TO SMELT A TON OF IRON.—In January, 1876, the Cedar Point Furnace at Port Henry, N.Y., made iron with a consumption of 1.26 tons of anthracite coal to a ton of pig iron. The Crown Point Furnace at Port Henry, N.Y., uses 1.64 tons of anthracite coal to a ton of pig iron; the Bay State Iron Company, at the same place, uses 1.33 tons of anthracite coal to a ton of pig iron, and the Fletcher Furnace at Buffalo, uses 1.399 tons of anthracite and .028 ton of coke, or 1.427 tons of the mixture to a ton of pig iron. In 1871, at the Glendon Ironworks in the Lehigh Valley, Pa., an open-top furnace used 1.19 tons of anthracite coal to a ton of pig iron; in 1872 the same furnace with a small cone top used 1.325 tons; and in 1873 the same furnace with a double cone top used 1.205 tons. The Thomas Iron Company, in the Lehigh Valley, Pa., in the last six months of 1875, use an average of 1.75 tons of anthracite coal (which at $3.41 a ton, cost $5.96) to a ton of pig iron; in the five years embraced in the period from 1869 to 1873 their average consumption was 1.978 tons of anthracite coal to a ton of pig iron. The Stewart Furnaces, in the Shenango Valley, Pa., use 1.891 tons of raw bituminous coal and .229 ton of coke to make a ton of pig iron, a total weight of 2.12 tons. The Brazil Furnace, Indiana, used, in 1872, 2¼ net tons of raw coal to a gross ton of pig iron. The coal then cost from $1.23 to $1.75 per ton at the mine. In 1872 the Fletcherville Furnace in Essex County, N.Y., used 0.95 ton of charcoal to smelt a ton of pig iron. The Deer Lake Furnace, No. 1, in the Lake Superior region, uses 1.073 tons of charcoal to a ton of pig iron; Deer Lake Furnace, No. 2, uses 1.097 tons of charcoal; Morgan Furnace, in the same district, uses 0.853 tons of charcoal; Bay Furnace uses 0.858 ton of charcoal; Fagette Furnace uses 1.040 tons of charcoal; and Elk Rapids Furnace uses 0.884 ton of charcoal. One of the furnaces of the Clarence Ironworks, England, uses 1.125 tons of coke to a ton of pig iron, and a furnace at Pouzin, France, uses 1.2 tons of coke to a ton of pig iron. Mr. William E. S. Baker, Secretary of the Eastern Ironmasters' Association, has published a table of the cost of the materials used in making a ton of pig iron each year from 1850 to 1875, in which he gives 2.859 tons of anthracite coal as the average consumption of fuel to a ton of pig iron at a furnace on the Susquehanna river in Pennsylvania; in 1875 the coal used to smelt a ton of pig iron cost $7.21. In 1875 Mr. John Fulton, then of Saxton, Pa., in the Broad Top coal field, estimated that to smelt a ton of pig iron would require 1.75 tons of Broad Top coke, 1.75 tons of Connellsville coke, or two tons of anthracite coal. At that time he stated the cost of each fuel at Harrisburg to be as follows:—Broad Top coke, $3.78 per ton; Connellsville coke, $4.25 per ton; anthracite coal, $4.50 per ton. The cost of the fuel used to smelt a ton of pig iron at Harrisburg was then as follows:—Broad Top coke $6.61½; Connellsville coke, $7.43¾; anthracite coal, $9.—*The Bulletin of the Iron and Steel Association.*

METALLURGY IN JAPAN.—The following is quoted by *Engineering* from a thesis prepared by a Japanese student, graduated at a United States college: Iron ores are very abundant in the Japanese Islands. The chief ores of Japanese iron industry are magnetic iron ore, specular iron ore, and brown hematite. The first is found in two varieties, one of iron-grey colour and the other black. Masses of this ore in the state of magnetic polarity, generally called lodestones, are found in the eastern part of Nipon, Sendai, and Nambu. They are very highly esteemed for the steel manufacture, for swords and compass needles. Japanese furnaces are small in size and simple in structure, although the principle is the same as that of the blast furnace used here and in Europe. The walls of the Japanese furnace are built with fire-proof clay, and sometimes with a few stones. The shape of the furnace is round at the bottom, having at one side an opening which is closed with a clay stopper. On the opposite side of the furnace wall, a little above the bottom, there are two openings through which a continuous stream of air is passed in the furnace by means of a Chinese bellows worked by men. Before the ores are put into the furnace they are piled up in heaps with coal and calcined, or roasted, so that the water, carbonic acid, and sulphur may be expelled. The Japanese do not know the theory of the puddling process used in the Western countries, but the principle is exactly the same. The cast iron mixed with some sand and some iron scales is melted with charcoal heat in a furnace similar to that already described, and kept in this melted state for several days until the whole mass assumes a fluid appearance. The Japanese method of steel making is entirely different from those usually employed in Western countries. It is done in this way. They mix a certain quantity of pig iron, which contains too great a quantity of carbon, with a certain quantity of bar iron, which has too little carbon, and cover the mass with borax and smelt in a small crucible of fire-proof clay for more than a week. The borax is used to dissolve any impurities in the slag. When the metal is separated from the slag floating on the surface, it is taken out and hammered hard, and alternately cooled in water and oil many times. After the steel has been cast in that method, it is cemented and tempered. The method of cementing consists in covering thickly the hammered steel with a liquid mixture of clay, loam ashes, and charcoal powder. When this layer is dried the whole is heated red hot and then cooled very slowly in warm water. The steel is now ground on a whetstone. The steel thus made is not very elastic, but is very hard. The explanation is that either the Japanese do not understand the tempering process, or they are unable to remove entirely the impurities from the steel. I have often heard Japauese blacksmiths say that watch springs can never be made in Japan, for Japanese steel is not

elastic. The Japanese take great care and time in steel manufacture for swords. For instance, for ordinary knives forging and cooling are to be done only four times, but for swords fifteen times. Copper is and will be the most important metal of Japan. It is found in almost every province. For roasting they have a furnace covered with a shed, provided near the bottom with several openings for the draught of the air. Five alternate layers of ore and wood are placed in the furnace and burned.—*Journal of Society of Arts.*

RAILWAY NOTES.

ON THE ADVANTAGES OF BRAKE BLOCKS OF CAST STEEL.—By Dr. Rohrig. The Ober-Schleswig railway has for some years used brake blocks of cast steel, or rather of an alloy of cast iron and cast steel, invented and manufactured by Glockner Brothers, of Tschirndorf, near Halbau. The advantages of this material, given as the result of a four years' experience, were as follows:

(1) A saving over wood in cost of maintenance amounting to 31 per cent. in wagons, and 39 per cent. in brake vans.

(2) The tires do not have flat places worn on them, which lessens the wear of the rails, as well as of the tires themselves.

(3) The damage done to the axle-boxes, &c., by these flat places is also saved.

(4) There is less danger of heating the tires, and the blocks cannot catch fire.

(5) The mechanism of the brake can be simplified, as the levers require less frequent adjustment.

Further experience has fully confirmed these results. The brake vans on the above railway fitted with these brake blocks have been found to run from 184,000 to 230,000 miles before the blocks need changing, whilst with wooden blocks the distance only averages 120,000 miles. The railway has now seven hundred and fifty wagons fitted with these blocks, and they have been largely adopted by other companies, especially the Uetliberg Railway Company, who report most favorably on their advantages for steep gradients. They are accustomed to send wagons down the incline without an engine at the rate of twelve miles an hour, relying on these brakes only, and with perfect security; whilst there is an absence of the jarring which is so unpleasant with ordinary brakes.

The only disadvantage is that the co-efficient of friction is somewhat less than with wood; but this can be got over by giving increased power to the gearing used for putting on the brakes.—*Abstracts from Journal of Institute of Civil Engineers.*

WE learn from our Indian exchanges that the construction of the Indus Valley Railway is making rapid strides towards completion. The chief engineer hopes to have the line open from Mooltan to Sukkur by the end of July. The line is now laid from Mooltan to Chowdree, in the Bhawulpore State, 113 miles, and by the end of this month it is hoped that it will be completed as far as Khanpore, 140 miles from Mooltan. The temporary bridge at Adamwahan still stands well, and arrangements are being made for a steam ferry against its giving way in the floods. On the Sukkur side, equally satisfactory progress is being made, Mr. Mackenzie, contractor, undertaking to complete sixty miles to Gobla within two months, being at the rate of a mile per day. Midway between Sukkur and Mooltan too, in the Khanpore section, work is being pushed forward, the material being sent by camel to the scene of operations there. Beyond Sukkur the work is not neglected, although the present effort is to open only as far as Sukkur. The line is quite complete from Kotree to Schwan, thus leaving only the short break to Sukkur from Schwan to be completed after the first effort has been successful. If the river behaves, there is every reason to hope that the line will be open right through within eighteen months from this date.

ENGINEERING STRUCTURES.

OBJECTION TO THE ALGERIAN SEA.—In a letter to M. Daubrée, M. Naudin states his fears that if the shallow Algerian *chotts* are filled with water, they will form an immense pestilential breeding place, worse than the Tuscan Maremma or the Pontine Marshes. The greatest depth in the centre of the basin would not exceed twenty-five metres, and over most of the surface the water would be very shallow. All the conditions would be favorable to an immense multiplication of organic life, decay and miasm.—*Comptes Rendus.*

THE CANAL WHICH THE GOVERNMENT HAS CONSTRUCTED to overcome the obstruction to navigation caused by the Des Moines Rapids in the Mississippi, at Keokuk, Iowa, was formally opened with suitable ceremonies last Wednesday. The canal extends along the Iowa shore from Keokuk to Nashville, a distance of seven and six-tenths miles, is 300 feet wide in embankment and 250 wide in excavation; minimum depth of water, five feet; maximum depth, eight feet, which is sufficient to float the largest steamers that ply the waters of the upper Mississippi. The embankment enclosing the canal is ten feet in width on top, with a rip-rap covering two feet thick and carried two feet above extreme high water mark. The fall in the entire distance which the canal extends is 1,875 feet. There are two lift-locks and one guard-lock, each 350 feet long and eighty feet wide on top. These are built of solid cut-stone masonry, and are pronounced by experts who have examined them to be very substantial and highly creditable specimens of engineering skill. Sluices of sufficient capacity to control the surplus water carried into the canal by the numerous streams emptying therein during their frequent floods, are built around the locks. These were not included in the original estimate and have materially increased the cost of the work. The machinery for operating the lock-gates and wickets is made from an original design of Major Amos Stickney, the officer in local charge of the improvement. This plan consists of a

system of pulleys, chains, and wire ropes, operated by means of a pump forcing the water into hydraulic cylinders sunk behind the walls back of each gate, connected by means of iron pipes, with an engine situated near the head of the lock, so that one man at the engine can handle the massive gates and wickets with ease and precision. The original estimate for this improvement was $3,390,000. It has cost so far $4,281,000. Work was begun on it in October, 1867. It is estimated that $100,000 will yet be required to finish the work, and Congress will be asked to make an appropriation of this amount at the next session. The improvement is of incalculable importance to the navigation of the Mississippi river, as it removes the only obstruction now remaining between New Orleans and St. Paul."—*Engineering News*, iv. 228.

FIRE-RESISTING COMPOUND ARCHED FLOORS AND ROOFS.—We have recently had an opportunity of inspecting a floor constructed on a principle which has been patented by Mr. George Northcroft, architect, of 5 Castle Street, Liverpool. The following description will enable our readers to understand the mode of construction, and appreciate the advantages of the system. When the walls of the building to which the system is to be applied are brought up to the floor level, flat centers are set, filling the entire space between the walls. On the side walls cast-iron wall plates are bedded, having cast seatings for expansion rollers, on which are set rolled iron girders, placed at such distances from each other, as is necessary, according to the span and load to carry. On the end walls other cast-iron wall plates are set, the four being secured at the angles, thus forming a continuous bond round the building. On the cast-iron wall plate along the end walls a fireclay skew-back, the full depth of the floor, is bedded. To each rolled girder is bedded in asphalte composite closely fitting fireclay skewbacks with radiated joints, which completely surround and protect the girder from the fire. Thus, divided into bays, the floor may be continued for an indefinite length. Fine mortar or cement is used in setting the brickwork filling the bays. The key of each bay, the full depth and width of the floor, three inches wider at the crown than at the soffit, is set first. The cross joints of this key are radiated from the center each way, hence what may be termed the keystone of this key is three inches thinner in the soffit and six inches shorter than at the crown. Between the keys running parallel to the skewbacks surrounding the girders, the space is filled in with two courses of 6 inch fire bricks, grooved and tongued into each other, one course kept uniform with the floor level and the other uniform with the ceiling, the space between being determined by the depth of the floor. These two upper and lower courses of bricks are also united in the middle of the floor by keys the full depth of it, which act at right angles to the keys of the bays. From these keys the joints of the 6-inch bricks radiate, thus producing a complete counterpoise, constituting the floor a series of compound arches. If great strength is required and the bearing long, the girders used are proportionately strong and deep, and set together. The patentee claims originality in his design, in the provision made for the complete protection, as well as the expansion and contraction, of the iron employed in construction; in the combination of indestructible material, whereby great widths can be spanned without any intermediate supports, and the floors made to sustain unusually heavy loads.—*Architect.*

ORDNANCE AND NAVAL.

SMALL ARMS FOR FIELD ARTILLERY.—The Martini-Henry carbine recently issued on a small scale to the Royal Artillery has proved itself, as far as early trials demonstrate, a most efficient weapon. It is highly satisfactory that this branch of the Service should be in possession of such an excellent small arm, if such a weapon is necessary for gunners at all. The attempt to convert a well-trained garrison artilleryman into a very indifferent infantry soldier, solely for considerations of "sentry go" and martial display at field-days and reviews, is such a time-honored custom that it fails to evoke any surprise or remonstrance from the most ardent of "true reformers." When we consider field artillery in connection with small-arms for their own defence, we cannot so readily dismiss the question; the course pursued in their case seems so full of vacillation and uncertainty. For many years the complement of carbines to a battery of six guns was twenty-four, four being thus at the disposal of each gun detachment. If we omit the mounted non-commissioned officers in charge, this was in the proportion of one to every two men. This number might, under exceptional circumstances, have proved sufficient for an emergency; but without the total abolition of the weapon its most ardent opponent would scarcely demand a reduction in the issue. Modifications in modern tactics, based mainly on the experience of the last great European war, have led to the gradual emancipation of the field artillery from the "escorts" which hampered its movements, and, where composed of infantry, almost wholly deprived it of its mobility. Field artillery, according to the latest theory and pratice, is to show itself practically unescorted in the forefront of the fight. In this position, should it escape annihilation from the oppsing small-arm fire, it is extremely liable to surprise or to the capture of its guns by a rush of infantry under cover, or by a cavalry charge. Such considerations point to the necessity for some last resource, some *pis al'er* in the moment of need. The escorts being abolished, surely the gun detachments should be more than ever capable of self-defence; and yet the proportion of carbines has been reduced one-half, two only being alloted to a gun detachment by the latest regulation. The French arm every gunner of a detachment of field artillery with a carbine, nor can we detect any marked slowness or awkwardness in the service of their guns. If it be held that a carbine slung on the body as carried by the French field gunner is liable to hamper a man in his actions, the intro-

duction of a pistol for gunners might prove desirable. When it was the custom to afford guns constant protection in the shape of the escorts now abandoned, it was essential that gunners should devote their exclusive attention to their own distinctive arm. As, however, they are now liable to be called upon to defend themselves in a *melee*, the question naturally suggests itself—are two carbines tightly strapped to a limber, with every regard to neatness, adequate provision for the defence of eight otherwise unarmed men ?—*The Broad Arrow.*

ON THE DIFFERENCE OF THE STEERING OF STEAMERS WITH THE SCREW REVERSED WHEN UNDER FULL WAY, AND WHEN MOVING SLOWLY. BY PROF. OSBORNE REYNOLDS. The author said the fact that the results which had been established by the Committe were so little known to pilots and seamen, besides being likely to excite surprise, would tend to cast a certain amount of discredit, if not on the truth of the results themselves, at least on their importance. It seemed as if nautical men must have formed their opinions from experience, and such was the faith of the English people in the practical that it was very difficult indeed for them to believe that a few landsmen, calling themselves scientific, could teach sailors how to steer ships. So strong was this feeling that it was to collisions they must look in the hope of preventing collisions. This sounded like a bull, but it was perfectly true, for nothing but disasters would awake our rulers to the idea that something was wrong. Fortunately, or unfortunately, such disasters were not wanting. There were the cases of the Ville du Havre and the Loch Earn, in which the collisions were undoubtedly due to the steamers having turned in the opposite direction to that intended. These and other disasters furnish evidence enough of the mistakes which had been perpetrated, and of the importance as well as the truth of the results the Committee had established. He fancied that the ignorance which existed was due to the fact that few seamen had turned a ship under full way with the screw reversed, and contented themselves by arguing as to what must happen in such a case from their experience in manœuvring their ships when moving slowly. Of such manœuvres they had had abundance, but as soon as they had got beyond their experience, they adopted the seemingly obvious, but entirely erroneous opinion that the way of the ship would cause the rudder to act as if she was going ahead in spite of the screw being reversed. He felt strongly that in speaking thus in a town like Plymouth he ran the risk of being looked upon as impertinent. If he were wrong he was impertinent, and no one would feel it more than he should. It was not a pleasant task to point out imperfections, however accidental they might be. Even if one saw the wheel coming off an omnibus, all the thanks he was likely to get for pointing it out to the conductor was to be asked if he could not tell him something he didn't know. Of course they must learn as they went on, and all he, with deference, asked of seamen was to try the experiments for themselves, and then aid the Committee in bringing facts under the notice of the Legislature. Their own interests demanded this, for as things now were, great injustice might be done to the captain, who, in a case of emergency, adopted the very best course to save his ship.

BOOK NOTICES.

TABLES FOR THE DETERMINATION OF MINERALS BY THEIR PHYSICAL PROPERTIES. By PERSIFOR FRAZER, JR., A. M. Philadelphia: J. B. Lippincott & Co. For sale by D. Van Nostrand. Price $2.00.

This is a new edition of a work already well known to mineralogical students. The tables are constructed with reference to detecting minerals by aid of such simple apparatus as the worker can carry into the field.

The minerals are first classified into groups depending upon lustre or color. Then against each mineral name are given other properties, as follows, viz., Lustre; Color; Streak; Hardness, Tenacity; Crystal System; Clearage; Chemical Formula; to which is also a *Remark* giving Specific Gravity, and generally a Blowpipe Reaction.

No other set of similar tables contains so much in so small a space.

WOODWARD'S ORNAMENTAL AND FANCY ALPHABETS. By GEORGE E. WOODWARD. New York: W. H. Stelle & Co. For sale by D. Van Nostrand. Price 50 cts. per number.

This work is issued in twenty parts, each being a quarto pamphlet.

The merit of the designs we take to be, that degree of distinctness or simplicity which admits of easy and prompt reading. This applies to the complex monograms which are too often offered as ingenious puzzles. The designs in the six numbers before us are elegant and beautifully printed.

WOODWARD'S NATIONAL ARCHITECT

in twenty parts contains working plans of residences for city and country, and afford excellent study for young architects. The pamphlets are uniform in size with the alphabets mentioned above. Price 50 cts. each part.

A TREATISE ON THE USE OF BELTING FOR THE TRANSMISSION OF POWER. By JOHN H. COOPER. Philadelphia: Claxton, Remsen & Haffelfinger. For sale by D. Van Nostrand. Price $3.50.

There is no subject relating to Mechanism upon which the information has been so vague and unsatisfactory as this one of Belting. The need for such a book as this has long been manifest. That the subject has been treated by competent hands in the work before us is abundantly indicated by the papers previously published in a contemporary journal, and by the appreciative comments upon them by practical men.

The contents of the present volume are: Rules and Data for Belting; Methods of Belt Transmission; Cements, Adhesive and Fastenings; Varieties of Belting; Strength of Belting

Leather; Experiments of Briggs and Towne on Leather Belts; Experiments on the Tension of Belts by Morin; Rope Transmission of Power; Frictional Gearing.

The work is to a large extent a compilation from various authorities, selected with judgment, and arranged with much care. Many of the sections are essays from scientific journals. Quite as many are practical formulas and suggestions contributed by mechanical engineers of wide experience.

The illustrations, eighty-three in number, are exceptionally good.

The book deserves a wide circulation.

MANUAL OF SURVEYING, FOR INDIA. Compiled by COL. H. L. THUILLIER, and LIEUT-COL. R. SMYTH. Calcutta: Thasker, Spink & Co. For sale by D. Van Nostrand. Price $21.00.

This treatise, of over eight hundred pages, contains much that is of use only to a beginner, and, unless that beginner is located in India, he may get all that this book affords him in a smaller and less expensive work.

A portion of the work treating of Geodesic Surveying, contains valuable information; but it is only a small fraction of the book.

The volume might be profitably divided into separate treatises, three at least, one of which would be of use or interest only to surveyors in India. The other two would be serviceable to surveyors of other portions of the globe, although unnecessarily voluminous for the purpose.

The maps, typography and illustrations are all excellent.

THE THEORY AND ACTION OF THE STEAM ENGINE. By W. H. NORTHCOTT, C. E. London: Cassell, Petter & Galpin. For sale by D Van Nostrand. Price $3.50.

The book is written for practical men, and contains the rules and formulas necessary for the successful working of steam motors. For the theory of Thermodynamics the reader is referred to special treatises on that subject.

The author presents very fairly and clearly so much of the theory of the generation and use of steam, as is in constant request by practical engineers.

It may be read with profit by those students desirous of acquiring a knowledge of this theory who have not the time for such works as Rankine's.

The book is neatly printed, and has a fair supply of illustrations, mostly, however, of indicator diagrams.

PRACTICAL HINTS ON THE SELECTION AND USE OF THE MICROSCOPE. By JOHN PHIN. Second Edition. New York: Industrial Publication Co. For sale by D. Van Nostrand. Price 75 cts.

Such a work as this is needed by every beginner at the microscope, whether he is intending to work or is seeking only amusement. The suggestions in regard to collecting and preparing objects are of a thoroughly practical character, and so plainly given that the merest tyro may follow them.

The sections of the book treating of the construction and use of the various grades of instruments, and their comparative merits, may be read with profit by experienced workers in Microscopy.

The illustrations are numerous and are fairly well executed.

MINUTES OF THE PROCEEDINGS OF THE INSTITUTION OF CIVIL ENGINEERS. London: William Clowes & Sons.

We have received the following papers from the Secretary of the Institution, Mr. James Forrest.

The Whitson Pumping Station. By Thomas Sullock Storke, A. I. C. E.

Tunnel Construction. By Arthur Earnest Baldwin, A. K. C.

History of the Modern Development of Water-Pressure Machinery. By Sir William George Armstrong, F. R. S.

Street Tramways. By Robinson Souttar.

Economical Method of Manufacturing Charcoal for Gunpowder. By George Haycraft, A. I. C. E.

RAILWAYS OF NEW SOUTH WALES. Report on their Construction and Working during 1876. By JOHN RAE, A. M., Railway Commissioner. Sydney: Chas. Potter. For sale by Van Nostrand. Price $4.00.

This is a voluminous Government report, abounding in statistical information of much value. It is a model of systematic arrangement, and is, therefore, a valuable addition to the literature of railway systems and their working.

The economical results are compared with the estimates of railways in the other Australian Colonies, as well as with those of the United Kingdom, and of this country.

In addition to the details common to such reports, profiles of the lines are given, together with a map of the Colony exhibiting the entire railway system.

We will present an abstract of this valuable report in our next number.

ASSOCIATION DU CHEMIN DE FER SOUS-MARIN ENTRE LA FRANCE ET L'ANGLETERRE. Rapports présentés aux membres de l'Association. Paris.

This is a collection of reports by different members of this "Association," presented to the Assemblée Générale on the sixth of June last.

M. Michel Chevalier, President of the Permanent Committee, is also acting President of the Association, and it is to the politeness of this gentleman that we are indebted for an early copy of the Rapports.

The publication includes:

1st. A Report on the Actual Condition of the Enterprise.

2d. A Financial Report on the work of 1876.

3d. Report of M. A. Lavalley, on the Results of Surveys, Soundings, etc.

4th. Report of M. Larousse, Hydrographic Engineer.

5th. Report on the Submarine Geological Explorations, by M. M. Potier and Lapparent, Engineers of Mines.

6th. Report on a Boring at Sangatte, by M. M. Potier and Lapparent.

The reports are abundantly illustrated with plans and profiles of the finest description. In a future issue we will present abstracts of 1st, 3d, 4th, and 5th Reports.

A MANUAL OF RULES, TABLES AND DATA FOR MECHANICAL ENGINEERS. By DANIEL KINNEAR CLARK. New York: D. Van Nostrand. Price $7.50

Books, like nearly all other things, may be divided into three classes: good, bad and indifferent. Mr. Clark's new manual may unhesitatingly be ranked in the first class. In general character it resembles such "pocket books" as Haswell's and Trautwine's, but instead of being a pocket book it is a large volume of 984 pages which measure 6 x 9 inches. It is printed on heavy tinted paper, and therefore it is a thick book, not at all resembling a "pocket book," In its general character it resembles Trautwine's book more than any other; but that, as most of our readers know, is a pocket book for civil engineers, whereas Mr. Clark's manual is intended for mechanical engineers. The two books resemble each other in the fact that instead of containing only rules, tables and memoranda, they each have what may be called dissertations on the subjects treated. Trautwine's book is, however, printed in very small type and on thin paper, so that it is comparatively small, though somewhat larger than the usual pocket book. Mr. Clark's manual refers to a very different range of subjects, as it treats of mechanical and not civil engineering, although, of course, some subjects are common to both. Perhaps no better idea can be given of the character of Mr. Clark's work than to quote the titles of the chapters. These are: "Geometrical Problems," "Mathematical Tables," "Weights and Measures," "Money—British and Foreign," "Weight and Specific Gravity," "Weight of Iron and Other Metals," "Fundamental Mechanical principles," "Heat," "Steam," "Mixture of Gasses and Vapors," "Combustion," "Fuels," "Application or Heat," "Strength of Materials," "Strength of Elementary Constructions," "Work or Labor," "Friction of Solid Bodies," "Mill Gearing," "Evaporation Performances of Steam Boilers," "Steam Engine," "Flow of Air and Other Gasses," "Work of Dry Air or Other Gas Compressed or Expanded," "Air Machinery," "Flow of Water," "Water Wheels," "Machines for Raising Water," "Hydraulic Motors," "Frictional Resistances."

The two chapters on "Geometrical Problems" and "Mathematical Tables" might not inaptly be called mathematics for practical men. They give, in fact, a summary of practical geometry, mensuration and trigonometry in such a concise form as every man actively engaged in engineering has often occasion to use and refer to. What he then wants is a book in which can be found in the shortest possible time exactly the fact or the theory he wants to use. For such persons these chapters are admirably adapted.

The chapters on "Weights and Measures" and on "Money" show the confusion which exists the world over regarding these matters. The advocates of the metric system have here a text from which they could preach a very forcible sermon. The portion referring to wire gauges is almost comic in its diversity. There are six different English wire gauges given. Besides these there are, we believe, several which originated in this country. So great is the confusion on this subject that, to express a dimension as such a number wire gauge would not mean anything definite, unless it was stated what special gauge was meant.

The tables of weights and specific gravity are unusually full. Of course, any careful examination of these tables is quite out of the question in a review of this kind, but from an examination of the tables and data it would seem as if almost any information which might be desired could be found under its appropriate head.

A similar remark might be made of the chapter on "Fundamental Mechanical Principles" that was used to describe the chapter on "Geometrical Problems." If the one is mathematics for practical men, the other may be called mechanics for the same class. The explanation of such matters as the "centre of gyration," "work accumulated in moving bodies," and "work done by percussive force," are remarkably clear and concise.

In the chapters on "Heat" and "Steam" will be found a condensation of all the latest information relating to these interesting subjects. Our remarks would assume the character of a table of contents if we undertook to give an idea of the scope of these chapters. They give in a very lucid and condensed form the substance of the information contained in larger treatises on these subjects, so that information relating to them is easily accessible with an expenditure of a very little time. The same thing is true of the chapter on "Combustion."

The subjects of "Fuels" is discussed very fully, and information is given regarding coal and other fuel from all parts of the world. To our shame be it said that no other information, worthy of the name, was procurable concerning American coals excepting that contained in Prof. Johnson's report made in 1843–44. The experiments which were the basis of his report were very complete, but since then large coal fields have been opened, and many qualities which were then unknown are now extensively used.

It is, in fact, difficult to describe the book without becoming monotonous in reiterating the same commendation of every part. Mr. Clark's plan seemed to be to take up the subjects treated and then seek all the attainable information relating to them and put it into as condensed a form as possible. The chapter on the "Applications of Heat" illustrates this, as all the others do. The transmission of heat through solid bodies is fully treated, then the heating and evaporation of liquids through metallic surfaces, the cooling of hot water in pipes, the condensation of steam in pipes, warming and ventilation, evaporation in the open air, desiccation and heating of solids.

The discussion of the "Strength of Materials" occupies 132 pages, derived chiefly from English sources, and is undoubtedly very valu-

able, but is deficient in not giving the results of German investigations, like those of Woehler, and others made in this country by Prof. Thurston. By representing graphically the action of material under strain, and not only its ultimate strength is determined, but other qualities, such as elasticity, ductility, etc., are clearly shown. The "autographic" method by which the material is made to draw an outline of its own character, promises, or rather has established, a new era in the investigation of the quality of materials.

The discussion of the "Strength of Elementary Constructions" contains the results of experiments made to determine the strength of rivet joints, columns, beams, railway rails, steel springs, ropes, chains, bolts and nuts, stay bolts, cylinders, tubes, pistons and boilers. There is also a brief discussion of the strains of frame work, such as cranes, girders, roofs, etc.

The treatment of the "Friction of Solid Bodies" is comparatively brief, and anyone in search of any complete information on this subject will look in vain here, as he will everywhere else, for it, so that the reader will probably renew his hope that the coming investigator who is to make this subject clear will soon begin his labors.

"Mill Gearing" has 40 pages devoted to it, which forms a very useful compendium of that art. Besides discussing the subjects usually found in the treatises on this subject, much information of comparatively late date, such as frictional wheel-gearing, rope gearing, etc., are discussed.

Probably no portion of the book will have more interest for railroad mechanical engineers than that which refers to the evaporative performance of steam boilers. The information which is condensed here is spread over a very wide field, and to the general reader is therefore inaccessible. Among other things it contains a report of the experiments of Woods and Dewrance in 1842 to determine the evaporative efficiency of locomotive boiler tubes. These same experiments were repeated by M. Paul Havrez in 1874. In both cases it was found that the first six inches of the tubes next the fire-box did more work than the remaining 60 inches of tube. The record is, however, provokingly silent with reference to the important point whether the experiments were made with a forced blast, which is the condition under which a locomotive boiler is always worked, or whether they were made with the draft of an ordinary chimney.

The following conclusion, stated by the author in this chapter, every master-mechanic should lay to heart and hang up in his office in the form of an illuminated text. It is that "*There can never be too much heating surface, as regards economical evaporation, but there may be too little; and that, on the contrary, there may be too much grate-area for economical evaporation, but there cannot be too little, so long as the required rate of combustion per square foot does not exceed the limits imposed by physical conditions.*"

The limits of this notice will not permit any careful examination of the chapters on the "Steam Engine" and on the "Expansive Working of Steam" "Flow of Air," "Air Machinery," "Water Wheels" and "Frictional Resistances." The much-disputed subject of the economy of the compound engine is discussed quite fully in the chapter on the steam engine, but to examine it critically would require more time and room than can now be given to it.

Altogether Mr. Clark's book may and should be commended very highly. It is vastly the best book of the kind ever yet published. It contains no mathematics with the exception of a little simple algebra and can be comprehended by the ordinary reader who is without the accomplishments of a knowledge of the higher mathematics. The book, it is quite safe to predict, will become the office companion of nearly every mechanical engineer who takes any pride in his profession.—*R. R. Gazette.*

L'AIR COMPRIME ET DES APPLICATIONS, PRODUCTION, DISTRIBUTION, ET CONDITIONS D'EMPLOI. Par M. A. PERNOLET, Ingenieur. Paris: Dunod, 1876. For sale by D. Van Nostrand. Price $8.00.

Scarcely used about thirty years ago, compressed air is to-day rightly regarded as a power transmitter and storer of the very first importance. The oldest application of compressed air is to the diving bell, mentioned by Aristotle in his "Problems." The diving dress was only invented about fifty years ago. Sinking bridge caissons into sandy bottoms, sinking wells by the use of compressed air, are other modern statical applications. As examples of what may be termed dynamical applications may be cited winding engines for working shafts and inclines; drills; pumping engines; and coal-cutters in mines and collieries. The cutting of the long tunnels of the Mont Cenis and St. Gothard, and of the Hoosac Tunnel in the United States, would have been next to impossible without the use of compressed air. The first-named work no doubt gave the impetus to this branch of engineering. In America compressed air is sometimes used for driving machinery in confined workshops. In the St. Gothard the locomotives for taking out the rubbish, when in the tunnel, are worked by compressed air. Inventors in Scotland and in France are now working at tramway locomotives driven by compressed air, and the Whitehead torpedo constitutes a sufficiently noteworthy invention. In ventilating large confined spaces, for instance, jets of compressed air are used to induce subsidiary currents by means of some form of injector. In London, Paris, Berlin, and Vienna compressed air is applied in the pneumatic despatch tubes; and air is the motive power of several forms of brakes now competing for the favor of the railway companies. Its purely physical properties, such as its elasticity, are employed in signaling, and its capacities for taking up heat and doing work are made use of in cooling and ice-making.

The treatise is divided into six parts. The introduction treats of the history, applications, advantages, and disadvantages of compressed air. The first division examines the general theoretical conditions of its production, distri-

bution, and use; the second relates to the production more especially, with descriptions of the construction and duty of most of the compressors actually in use, comparing them together at the same time. We then come to an examination of the modes of distributing compressed air, the piping, reservoirs, and accessory apparatus, to its actual practical applications; while the fifth and last division treats on the economical conditions involved in its employment. At the end of the description of each machine the excellent plan is adopted of giving in a short recapitular table the principal dimensions and numerical data of each machine and its engine.

He divides compressors into four principal classes (1) low, (2) medium, (3) high, and (4) very high pressure, according to the pressures, (1) less than one atmosphere, (2) from 1 to 3 atmospheres (3) from 3 to 7, and (4) above seven atmospheres, at which they work. This is quite correct, as it is the working pressure which most influences the physical conditions of working and the suitable mode of construction. Compressors working at moderate pressure supply air for diving, and to the caissons of bridges; Bessemer blowing engines, propelling air at about 20 lb. above the atmosphere, the pressure required to counterbalance the ferrostatic column, come under the same category. In the description of the compressors built by Cail and Co., for the contractors of the Kehl bridge over the Rhine, and set up in order to inject compressed air to equilibrate the head of water below which the men were working in the caissons, it is stated that these compressors offer the first instance of an injection of water. This is a mistake, and we could point to more than one British specification, several years anterior to that date, embodying this process; and not merely, as in this case, injecting it into the air-chest, but directly into the compressor.

The title of this work does not do full justice to its contents. In the first place, information is given not merely relating to the compression, but also, as should be, to the expansion, in order to do work, of air; and, in the second, not merely the use of air is treated on, but also of other gases, such as lighting gas for portable use, and of carbonic acid gas in the continental beet-root sugar refineries. Without much increasing his volume, the author could well have brought the expansion of air more within the range of his work; or, in other words, have given the few instances extant where rational attempts have been made to re-expand the air, returning it the heat it requires for expansion. Then, not sufficient is said about the means or apparatus adopted for separating the water from the air, when direct inter-contact has been employed to keep down the temperature generated by the compression. This possibly results because there is no recognition whatever by the author of the important physical fact that water absorbs very considerable volumes of air—volumes dependent on the pressure of the air and the amount of surface of water exposed to the fluid contact, time being also no doubt an important factor. What this book—excellent as it is—lacks is perhaps a more detailed criticism based on actual examination of the machinery while at work. No doubt such information is difficult and costly to get; but several of the machines herein described, and even favorably noticed, are within our own knowledge working most execrably. Nevertheless, we repeat that it is the most important and complete work on the subject, affording an excellent point of departure for further investigation and improvement. What is at present required in the use of compressed air is a considerable diminution in the first cost of obtaining the compressed air by really improving the compressor, and a practical means of working the air at a high rate of expansion without the present attendant losses. It is certainly not too much to say that the air compressor of the future has yet to be invented. —*Engineer.*

MISCELLANEOUS.

PROF. MORITZ has published an account of a thermometer he invented about thirteen years ago, which he now thinks might be of utility in meteorology. It is a nearly circular band of silver and platinum, fastened at one end, and at the other moving a mirror, the reading being taken from a scale on the mirror, as in a magnetometer.

AT a recent meeting of the Dumfries and Maxwelltown Water Commission, the petition of the inhabitants of Lochrutton, complaining of their sewage running into the loch for lack of drainage, and polluting their water supply as well as that of Dumfries, was considered; and it was resolved to intimate to the local authorities of the parish that unless they rectify the evil, proceedings will be taken against them.

THE provisional order of the Local Government Board for the formation of a united drainage district for Birmingham and the Tame and Rea district has been read a first time in the House of Lords, and copies have been supplied to the various authorities embraced in the scheme. It is understood that after the second reading a week's notice has to be given before it is considered by the committee to whom it will be referred, in order to give time for opposition to the measure.

A GLASS has lately been made with phosphate of lime, by M. Sidot. He states that it is perfectly transparent and very refringent (its index of refraction is 1.523, that of common glass being 1.525); and it can be worked like ordinary glass. It does not, like ordinary glass, dissolve all metallic oxides, but it dissolves very well oxides of cobalt and chromium. It is attacked by boiling acids, as also by potash; it is not attacked by hydrofluoric acid; and this property may render it valuable in employment of telescope glasses, for workmen who are exposed to these vapors, and who have to work in the art of engraving on glass.